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FUNCTIONAL DISEASES OF WOMEN: Cases Illustrative of a New Method of treating them through the Agency of the Nervous System. Also an APPENDIX, containing Cases Illustrative of a New Method of treating Epilepsy, Infantile Convulsions, Paralysis, and Diabetes. 8vo, price 2s. 6d. London: 1863.

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CHOLERA CURABLE:

A DEMONSTRATION OF THE

CAUSES, NON-CONTAGIOUSNESS,

AND

SUCCESSFUL TREATMENT OF THE DISEASE.

BY

JOHN CHAPMAN, M.D.,

M.R.C.P., M.R.C.S.,

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PREFACE.

THE doctrines concerning the essential nature, the causes, and the treatment of diarrhœa and cholera explained in the following pages, were first expounded in an essay, published in the *Medical Times and Gazette*, July 29, 1865. That essay, with considerable additions, was published separately in the following month, and was soon afterwards republished in the United States.

In October of the same year, and again in July of the following year, an opportunity was afforded me of testing the correctness of my views, and the efficacy of the treatment which they dictate. The experience which I then gained proved the soundness of the former, and the success of the latter. The English edition of the essay just mentioned, being speedily exhausted, a second and greatly enlarged one, incorporating the results of that experience, was published in the autumn of 1866, under the title: "Diarrhœa and Cholera; their Nature, Origin, and Treatment through the agency of the Nervous System." In November 1884, cholera, in its epidemic form, appeared in Paris, and afforded me another opportunity of making trial of the therapeutical method in question, and of submitting my hypo-

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thesis of cholera, together with the facts and arguments on which it is based, to the consideration of one of the most able and distinguished members of the Paris Faculty of Medicine—Professor Peter, Physician to the Hôpital de la Charité. I shall always feel especially grateful to him, not only for his kindness in confiding to my direction the treatment of twelve cases of cholera, but for the very cordial and friendly manner in which he ensured, as far as practicable, that my treatment should be effectively tried, and, above all, for his public and frank recognition at the Paris School of Medicine of the truth of the doctrine, and the success of the treatment, explained in the following pages.

It seems to me desirable that both the medical and lay public should become acquainted with the results of the experiment at the Hôpital de la Charité, and also with the judgment pronounced upon them by a man so fully qualified to judge, and so professionally eminent, as is Professor Peter. I have, therefore, been induced to publish a summary of the book mentioned above, and to add to it an accurate report of the cases treated in Paris, as well as of Professor Peter's appreciation of them, expressed in his lectures on Pathology at the School of Medicine.

Moreover, there are several interesting and important questions connected with the pathology of cholera, which are not adverted to in my book on the subject, published in 1866; for example: What is the immediate cause of the presence of sugar in the urine of many choleraic patients? What is the pathological significance of the ruby-like colour producible in the urine of many choleraics by the addition of nitric acid, as well as of the "typhoid symptoms" of cholera, and how are these produced? What is the meaning of the

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established fact—viz., that many more males than females are destroyed by cholera? What is the *real* significance of the comma-shaped microbe, and why does it abound in cholera patients? These questions, I believe, have not hitherto been answered; indeed, I doubt if they have been propounded. I have endeavoured to answer them in the following pages. My answers seem to me the true ones; and certainly each of the explanations given contains an additional argument in support of the hypothesis which, as I maintain, affords a clear and consistent account of all the phenomena of cholera, and indicates how it may be successfully treated.

The answer to the question: What is the pathological significance of the typhoid symptoms of cholera, and how are they produced? will be found, I believe, to deserve especial attention. About a fourth part of the whole number of persons who are destroyed by cholera, die during what is called the period of reaction, and a large proportion of these exhibit typhoid symptoms, which are generally regarded as evidence that cholera is originated by a blood-poison. Now, while supplying an explanation of the production of these symptoms, I have shown how their cause can be annulled, and how, therefore, the lives of those who would otherwise become victims of the typhoid form of the disease may be saved.

It is now just twenty years since the hypothesis explained in this volume was first announced. Its acceptance by the profession has, of course, been tardy and partial; I say, "of course," for the faculty of medicine, as well as the faculty of law, is, it is well known, especially conservative; therefore, when a doctrine so revolutionary, as is the one in question, of prevailing ideas and practices in respect to cholera, is propounded, what can be reasonably expected

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from a conservative faculty but conservation? But just as the conservatives, like the poor, are always with us, so too, in every department of life, there are a few active spirits more courageous than the rest, who dare to inquire and think for themselves—to "try all things, and hold fast that which is good;" and such men have not been wanting, in respect not only to the hypothesis of cholera expounded in the following pages, but in respect to the much larger subject, which indeed includes that hypothesis—viz., that of the causal relation of the nervous system to diseases generally, and the possibility of their successful treatment through the agency of that system.

Many physicians who have given little or no attention to the neuro-dynamic pathology, etiology, and treatment of cholera, have, nevertheless, been induced to consider more or less superficially, or profoundly, those general principles of neuro-pathology and neuro-dynamic medicine, which enable them to effect cures either otherwise impossible, or with a swiftness and completeness unattainable by means of drugs. The numerous reports which medical men in different parts of the world have been good enough to send me of cures wrought under the guidance of the therapeutical principles, and by means of the practice, which I have introduced—cures considered by them sufficiently interesting or important to be recorded and communicated to me—would fill a large volume. These reports have come to me not only from England, Ireland and Scotland, but also from France, Belgium, Germany, Australia, British Guiana, Canada and the United States. These encouraging facts lead me to hope they are an earnest that before long the medical world will appreciate and make use of the power, already within their reach, by

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which that dread plague, cholera, may be disarmed of its terrors and placed within the category of curable diseases.

While assured that if Spanish physicians were but familiar with the contents of this little volume, or even merely with the therapeutical treatment which it counsels, and were to adopt that treatment, the thousands of lives which cholera has already destroyed, and the thousands it is still destroying in Spain, might have been saved, I feel painfully conscious of my own powerlessness to help in saving only a few of the lives that are being lost. No doubt that treatment and its authenticated results might become speedily and widely known by the agency of the medical and lay press; but the press cannot be thus employed until its editors become, if not convinced of the truth of the hypothesis of which that treatment is the logical outcome, at least convinced that the treatment is successful. Those, however, who know editors best, know only too well that, as a class, they are—and justly so—the most sceptical of the human race. It seems then, that even though that treatment might avail to stay the ravages of cholera now desolating Spain, the obstacles to its speedy application are insurmountable, unless some plan—unhappily not yet visible in the horizon of possibilities—be immediately devised and executed by which the Medical Profession of Spain and the Spanish people can be swiftly rendered cognizant of the efficacy of the remedial method in question.

Nature casts on her children the responsibility of educating themselves, and punishes ignorance as inexorably as she punishes vice. Sphinx-like, she propounds her terrible riddles to the passing generations of men, who must answer them or be destroyed. A survey, however defective, of the

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vast amount of destruction of human beings, to be counted by millions, which has been wrought by cholera alone during even the historical era, is appalling; and until a satisfactory answer to the problem—What is the essential nature and what are the causes of this disease?—is found, this destruction will go on. Believing that that answer is contained in this volume, I cherish the hope that its publication may conduce to the lessening of human suffering.

JOHN CHAPMAN.

PARIS: 224 RUE DE RIVOLI,
July 29, 1885.

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CONTENTS.

SECTION I.

Introduction : Theories concerning Cholera.

	PAGE
In India, Cholera generally believed to be Non-contagious	1
In Europe, Cholera generally believed to be Contagious	2
Unproven Assumptions of the French Academy of Medicine	3
The Question, "Is Cholera Contagious?" reopened	3
The numerous Hypotheses of Cholera antagonistic to each other	4
Dr. Klein's Hypothesis	4
Dr. George Johnson's Hypothesis	6
Sir Joseph Fayrer's Conclusion	7
The Value and Necessity of Hypotheses	7
Sir Andrew Clark's Opinion of the Hypothesis explained in the following pages	7
Verifications of this Hypothesis	7-8

SECTION II.

The Nervous System : Cerebro-spinal and Sympathetic.

Functions of the Spinal Nerves	8
Disposition of the Sympathetic	8-9
Bernard's Discovery of the chief Function of the Sympathetic	9
The "Frigorific" Nerve	10

SECTION III.

The Proximate Cause, or Essential Nature, of Cholera.

Cholera a Disease of the Nervous System	11
The Rôle of the Spinal Cord in Algide Cholera	12
The Nature and Origin of the "Rice-water" Stools	13
The Temperature within the Rectum in Cases of Algide Cholera	13
Morbid Conditions of the Voluntary Muscular System produced by the Spinal Cord	14
Morbid Conditions of the Involuntary Muscular System due to the Sympathetic	14
The Cause of the Corpse-like Coldness, or Algide Symptoms of Cholera	15

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PAGE
Death of the Nervous System 15
The Rise of Bodily Temperature before and after Death: how produced . . 15
The State of the Spleen 17
Changes in the Aspect of the Skin after Death 17
Movements of the Limbs after Death: how caused 17
Early Onset of Rigor-Mortis 17
State of the Blood of Patients dying in Collapse 18
The Presence of Sugar in the Blood in cases of Cholera: its Cause explained . 19
The Retention of the Biliary and Urinary Constituents in the Blood: how effected 22
The "Typhoid Symptoms" of Cholera: how produced 22
Adequacy of the Hypothesis propounded to explain all the Phenomena of Cholera 23

SECTION IV.

The Remote, Predisposing, and Exciting Causes of Cholera.

Classification of the Causes of Cholera 24
Solar Heat 25
Wide Ranges of Temperature along with a High Temperature . . . 29
Disturbances of Atmospheric Electricity 30
The Absence of Ozone 33
Lowness of Site 35
Bad Food and Eating to Excess 36
Alcoholic Drinks 36
Opium 37
Purgative Medicines 38
Painful Dentition 39
Noxious Effluvia 40
Impure Water 40
Nocturnal Influences 41
Fear 42
Insanity 43
Sex: Why Cholera is more destructive to Males than Females 43
The Nature and Mode of Action of the several causes of Cholera negative the existence of a Cholera-poison 47

SECTION V.

The Non-contagiousness of Cholera: Dr. Koch's Microbe—possibly a Beneficent Agent; why it abounds in Cholera Patients.

Attendants on Cholera Patients rarely attacked by the Disease . . . 48
Visitors to a Hospital of Cholera Patients do not contract the Disease . . 50
The Washers of the Linen of Cholera Patients seem to suffer no more than other people within a region in which Cholera is epidemic . . . 51
The Men employed to remove the Dejections of Cholera Patients enjoy an almost complete Immunity from Cholera 51
Post-mortem Examinations and Dissections of Cholera Corpses do not induce the Disease 51

Digitized by Google

PAGE
Cholera cannot be produced in Animals by Feeding, Inoculating or Injecting them with Matters ejected from the Stomach and Bowels of Cholera Patients 52
Animals are liable to suffer from Cholera 52
The Sanitary Commission in India found no evidence of a Specific Poison in Cholera Excreta 53
Negative Results of the French Commission sent to Egypt 53
The Comma-shaped Bacillus: how, according to Dr. Koch, it produces Cholera 54
Results of the Investigation conducted in India, by Request of the Government, concerning the Etiology of Asiatic Cholera 55
Dr. Koch's Theory refuted 55
Comma-shaped Bacilli found in the Mouths of healthy Persons, and in the small Intestines of Persons who have died from Ordinary Diseases . . 56
The Comma-shaped Bacillus not the Cause of Cholera 57
Possibly a beneficent Agent 58
Why Comma-shaped Bacilli are especially abundant in Cholera Patients . 58
The Generation of Comma Bacilli 61
Summary Statement of what is known concerning the Comma Bacillus . . 61
Various Kinds of Comma Bacilli, and their Relation to each other . . . 64
The production of Comma Bacilli in the Intestines of Monkeys as a Result of artificial Inflammation 66
A morbid State of the Intestine, like that characteristic of Cholera producible artificially by irritating the Solar Plexus 67
Dr. Ferrán's Pretensions 67
The State of the Blood Globules in the Algide phase of Cholera 68
Some Blood Globules assume a morbid State, some remain healthy 68
A Knowledge of the State of the Blood Globules an important help to Prognosis 68
The morbid Conditions of the Blood Globules not caused by a Blood Poison . 70
Its real Causes: Hydro-dynamic and Neuro-dynamic 71
The "Nervous Theory of Cholera," pronounced by the Marseilles Cholera Commission to be that which best explains the "Pathological Physiology" of the Disease 72
Reasons for dissenting from some of the Explanations of the Commission . . 73
Facts illustrative of the Mode of Onset of Cholera 74
Cholera often ceases suddenly 76
The Origin of Cholera at Marseilles in 1884 77
Spontaneous Origin of Cholera in Syria and in Paraguay 77
Cholera is not spread by the Removal of Persóns suffering from the Disease to healthy Situations 78

SECTION VI.

The Prevention and Avoidance of Cholera.

Some Factors of Cholera not avoidable, some avoidable 79
Relative Potency of the several Factors in different Seasons of the Year . . 80
Causes of Cholera avoidable at any Time 80
Measures tending to avert Attacks of Cholera not likely to be adopted by the present Generation 81
Significance of repeated Visits of Cholera to one and the same Place . . 81
Inhabitants of the Foci of Cholera in Low Situations, and on the Borders of Rivers would best secure their safety by leaving them 82
The Cosmical, or unavoidable, Causes of Cholera 82

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SECTION VII.

Disinfection and Quarantine.

PAGE

Healthy Persons cannot be protected from Cholera by being prevented from coming in Contact with Choleraic Patients 83
Railway Passengers and Luggage fumigated 84
Fumigation, Land Quarantine, and Sanitary Cordons declared useless . . 84
Regulations of the Sanitary Conference at Rome. 84
Conferences for increasing and diffusing correct Ideas concerning Cholera desirable 85
Heterogeneous and chaotic Elements of the Conference at Rome in 1885 . 85
Agreement of its Members impossible 86
Absurd and disastrous Effects of incorporating crude Theories in the form of obligatory Regulations 86
Quarantine, like sanitary Cordons, useless 86
Tactics of the Supporters of Quarantine 87
Motives of the English Opponents of Quarantine discovered and revealed by the Cholera Commission of Marseilles 87
The Risk incurred by Opponents of Quarantine 88
"Those Demons called 'Englishmen'" 88
"The Large Body of Facts" which condemn Quarantine 89
Governments ought to encourage the Inhabitants of Localities which are Foci of Cholera to leave them 89
Precautions to be observed 89

SECTION VIII.

The Medicinal Treatment of Cholera.

Proportion of Deaths following the use of different Drugs 90
Injurious Effects of Drugs 91
Powerlessness of Drugs to rescue Patients from choleraic Collapse . . . 91
Medicines which remain inert during Collapse often become dangerously energetic during Reaction 91
Calomel, Castor-oil and Sulphuric Acid, especially dangerous . . . 92
Medicines likely to be the least harmful and most beneficial 92
During the early Phases of Cholera Medication most effective . . . 93
Medicines often fail to stop even the premonitory Diarrhœa 93
The Desideratum 93

SECTION IX.

The Neuro-dynamic Treatment of Cholera.

Cholera being a dynamic Disorder, is likely to be cured by a dynamic Remedy 93
The Discovery that the Energy of the Nervous Centres is lessened by Cold and increased by Heat, applied along the Spine 94
This Discovery renders Cholera curable 94
Scientific Character and wide Range of Application of the neuro-dynamic Treatment 95
The Dawn of a new Era in Pathology and Therapeutics 95

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SECTION X.

Directions for the Treatment of Diarrhœa.

PAGE

Removable exciting Causes to be found and removed 96
Simple Diarrhœa 96
Severe or choleraic Diarhœa 96

SECTION XI.

Directions for the Treatment of Cholera.

What the successful Treatment of Cholera by the neuro-dynamic Method implies 97
The Selection of a suitable Spinal Ice-bag in each Case 97
How the Spinal Ice-bag should be applied 97
Treatment when the Vomiting subsides before the Purging 98
How the extreme Thirst during Collapse should be allayed 99
Treatment when the Purging subsides before the Vomiting 99
Nervine Stimulants to be avoided 99
The Patient's Life mainly dependent on the Nurse in Attendance . . . 99
Laborious Duties of the Nurse 99
First-class Nurses especially needful 99
Treatment during the Stage of Re-action 100
Great, and therefore dangerous, Power of the Spinal Water-bag in the Treatment of Cholera 100
The Spinal Water-bag may re-produce Vomiting and Purging . . . 101
Chief Requirements of Patients during their Convalescence 101

SECTION XII.

Points in respect to Treatment still Undetermined.

The Administration of Oxygen 101
Effects of its Administration as the sole Treatment 102
Proposed Use of Oxygen and of the Spinal Ice-bag *Simultaneously* . . 103
The Application of Heat over the Surface of the Body, and the Supply of hot Drinks during the Algide Stage 103
Reasons for giving Cold Drinks during the Algide Stage 103
Reasons for placing Patients when in the Algide Stage in Cold Air Rooms . 104
Cases in which Cold Affusions were seemingly beneficial 104
Cases treated successfully at Malta by Ice applied simultaneously over the Stomach and along the Spine 105

SECTION XIII.

Success of the Neuro-Dynamic Treatment of Cholera.

Statistical Results of the neuro-dynamic Treatment of Cholera at Southampton 106
Characteristic Therapeutics—Effects of the Treatment 106
It stops Cramps, Vomiting and Purging 106
It conduces to Sleep 106

Digitized by Google

PAGE
It restores the Pulse 107–8
It makes the Patients warm 107–8
Success of the Treatment reported by Medical Witnesses . . . 108–9
The Author's Attempt to treat Cholera in Paris in 1865 109
His Treatment of twelve Cases at the Hôpital de la Charité, Paris, in 1884, by Permission of Professor Peter 110
Reports of those Cases 111
Proportion of Deaths to Recoveries 116
Alleged Mildness of the Cases 116
Average Mortality of Cholera Cases in Europe 117
Proportion of Deaths to Cases in Paris in 1884 117
Proportion of Deaths to the Cases Treated in the Hôpital de la Charité in the Manner ordinarily practised there 117
Severity of those Cases compared with that of those treated by the neuro-dynamic Method 118
Proportion of Deaths to the whole of the Cases treated by that Method . . 118
Different Groups of Symptoms in Different Cases 118
Why different Groups present themselves in different Cases 119
The relative Rapidity with which Vomiting and Purging are arrested in different Cases 120
Practical Verification of the neuro-dynamic Theory of Cholera . . . 120

SECTION XIV.

The Analogy between Cholera and Sea-sickness.

The Phenomena of both Diseases similar 121
The ultimate Cause of both Diseases similar 121
Both are Diseases of the Nervous System 121
Both Diseases curable by one and the same Remedy 121
Cholera no more the Result of a Blood-poison than is Sea-sickness itself . . 121

SECTION XV.

Public Declaration in the Paris School of Medicine that Cholera is a Disease of the Nervous System, and that the Neuro-Dynamic Treatment of it is Successful.

Early Recognition of the neuro-dynamic Hypothesis by Dr. Clifford Allbutt, Dr. Maudsley, and Sir Andrew Clark 122
The Hypothesis in comparative Obscurity during Twenty Years . . . 122
Professor Peter's Appreciation of it 123
He affirms that the neuro-dynamic Treatment is successful, and recommends its Adoption 124

SECTION XVI.

Summary and Conclusion 125

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CHOLERA CURABLE.

SECTION I.

Introduction: Theories Concerning Cholera.

In India, the so-called "home" and "source" of cholera, where it is generally believed to be non-contagious, its presence produces no panic: the healthy relatives or friends of the sick minister to their needs without fear of becoming infected; the physicians and nurses, on whom they are dependent for treatment and care, have as little apprehension of danger, when attending to them, as they feel when attending to other patients; victims of cholera are not isolated, but, on the contrary, it is even customary to treat them in the same wards in which sufferers from other diseases are treated, and without any evil result; the clothes of such patients are not burnt, or even fumigated; all persons are free to go in, or go out, of cholera districts without let or hindrance; and the disinfection of passengers at railway stations as well as the use of *cordons-sanitaires* is practically unknown.

In Europe how widely different are the beliefs and consequent customs which prevail! The doctrine that cholera is contagious is everywhere held and taught authoritatively by physicians; it suffuses the whole medical press; it is generally echoed by the lay press of every country; and it is accepted without question as an article of faith by all European peoples. While the last great discovery of the alleged proximate cause of cholera—the comma-shaped microbe of Dr. Koch—is already subsiding into the Lethe which, during the last fifty years, has engulphed hundreds of fungi and animalcules which had played

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a like *rôle*,* the hypothetical *cholera contagium* holds its own, and is spoken of undoubtingly as a zymotic (fermentative) agent, capable of multiplying "in a ratio at least as great" as that of small-pox; it is dogmatically affirmed to travel from one district or from one country to another; to proceed, as a general rule, along the lines of human intercourse and, especially, along the courses of rivers; to be sometimes favoured by the wind, but not infrequently to advance in opposition to it; and, most certainly, to avail itself of ships in order to pass from one country to another separated by sea. When cholera appears in any town or village its inhabitants are seized with panic and seek safety in flight; contact with victims of the disease is dreaded (even by some medical men)† as if fraught with the utmost danger; they are isolated from the healthy; their clothes are burnt and, sometimes, also, even the things they may have handled,‡ or the carriage in which they may have been conveyed. *Cordons-sanitaires*, transgressors of which run the risk of being shot,§ are often placed round the "infected" districts; letters in transit are stopped and fumigated; if the rigour of the *cordon* is so far relaxed as to allow trains to pass, the passengers are fumigated also; ‖ and, finally, in the hope of preventing the importation of the disease from one country to another, the several nations of Continental Europe enforce the useless and vexatious practice of quarantine.

Every reflecting person who duly considers the wide difference here indicated between the beliefs and practices of India on the one hand, and those of Europe on the other, in respect

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* A brief account of these is given in Aitkin's "Science and Practice of Medicine;" sixth edition, art. on "Malignant Cholera;" but whoever is anxious to satisfy his curiosity on this subject may do so by consulting the legion of books concerning it, mentioned in the "Index-Catalogue of the Library of the Surgeon-General's Office, United States Army," vol. iii. Washington, 1882.

† "The two doctors Succi and Poli, who a few days ago refused to attend a woman here in Rome supposed to be attacked by cholera, have been sentenced, the one to 100 francs fine and three months' suspension from the exercise of his profession, the other to 60 francs fine and one month's suspension."—*Times*, Sept. 22, 1884.

‡ In the letter of the *Times'* correspondent published on Sept. 12, 1884, he says: "A friend of mine who has just returned from Naples, told me that he saw a fruit woman tumble off her seat in the Mercato, and as she was carried away a bonfire of her chair, stand, and fruit was made on the spot." Railway carriages conveying passengers, who while in them were seized with cholera, were, in 1884, burnt at Spezia.

§ At Spezia, the soldiers were ordered to shoot any one attempting to pass the cordon.—*Times*, Sept. 10, 1884.

‖ The King of Spain, on returning from his visit to the hospitals and cholera patients at Aranjuez (July 2, 1885), was fumigated before he entered the railway station at Madrid.—*Times*, July 3, 1885.

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to cholera, must be struck with astonishment, as it appears to me, by this marvellous discrepancy. And yet how few European physicians seem ever to consider it, and how rarely any one of them is led to doubt the truthfulness of his own convictions, and to ask himself whether there be any real grounds for those precisely opposite convictions which prevail in India! At the sitting of the French Academy of Medicine, August 26, 1884, a report was read concerning a proposal to organize a Commission for the study of the phenomena of cholera. In this report occurs the following statement:—"This [proposed] inquiry, it must be borne in mind, is not inspired by a preconceived doctrinal idea;" nevertheless, in this same report it is said that two of the principal objects of the Commission would be "to determine the length of the period of incubation of cholera, and whether it has been imported into the towns and villages of France, until then free from it, by man or by contaminated objects." It is evident from these words that the Commission of the French Academy of Medicine started on its course of investigation already loaded with three assumptions—viz., (1) that cholera is the consequence of a specific poison; (2) that this poison is zymotic, and therefore has a period of incubation; and (3) that it is imported from without into towns and villages previously free from it! That a body of scientific men, while holding firmly to these assumptions as if they were proven verities, can complacently assure itself that it "is not inspired by a preconceived doctrinal idea," is at once an astounding example of self-delusion, and an instructive proof of the great depth and strength of the current of prejudice which has to be drained off before European physicians as a body will become able to investigate the nature of cholera in accordance with that scientific method now recognized as the only sure guide to truth.

But, notwithstanding the assumption of these propositions as axiomatic truths by a Commission of scientific men—members of the authoritative medical body which directs the thought and moulds the belief of France on medical subjects, I venture to re-open the question—Is cholera contagious? On the answers which are given to this question hang great practical consequences. All European Governments, advised by their several medical counsellors, answer it, through their accredited organs and by their actions, in the affirmative; the Government of India, also advised by its medical counsellors, whose minds are most informed by experience of the matter, and who therefore may fairly be presumed to be the most competent judges, answers it in the negative. In thus advising the Government of India, Anglo-Indian physicians rely mainly on experience;

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for up to the present time no investigator of the nature of cholera, where it may be most continuously observed, has been able to reveal to us in what consists its essence and cause. On the other hand, many theories concerning the etiology of cholera have been propounded, each differing from the others, and each advocated by its author with a confidence in its truth not seldom provocative of scepticism in the minds of impartial students of the disease. Whoever compares those theories with one another is astonished by their mutual antagonisms; but he is still more astonished in observing the large extent to which their several bases are products of the imagination. If, severally, they afforded, though in different ways, explanations of each, or even of the majority, of the numerous phenomena of cholera—explanations consistent with each other and developed from one and the same common postulate—they might deserve, and even command, the respect of every logical scientist; but, unfortunately, they are, as a general rule, singularly devoid of these needful characteristics. Those authors, of whom there are several, including Sir Wm. Gull and Dr. Copland, who have taught that cholera is due to a profound depression or diminution of the vital force of the Sympathetic, have failed to explain what are the successive links in the chain of causation between that morbid state and the production of the several symptoms of cholera, and have equally failed to explain by what agency the Sympathetic is thus profoundly depressed. During the discussion on cholera, at the Royal Medical and Chirurgical Society, London, begun by Dr. George Johnson, who read a paper on the subject, March 24, 1885, Dr. Klein, who has distinguished himself as an opponent of the theory of Dr. Koch, made the following announcement:—

"Cholera, when it appears and spreads in Europe, is the result of importation from an infected locality. . . . . The infective agency must be a living entity, since it is possessed of the power of self-multiplication. . . . . It does not enter the body, but lives and multiplies outside this, and as a result of its activity produces a chemical ferment which when introduced into the system acts as the virus. . . . . The whole course of the disease and the symptoms eminently favour the theory that it is due to a chemical ferment acting on the blood, the nerve-centres, and the organs of circulation."*

Notwithstanding my great respect for Dr. Klein, I am constrained to say that thousands of well-authenticated facts concur in negativing these assertions. No evidence has been adduced of the existence of a "chemical virus" which "acts on the blood, the nerve-centres, and the organs of circulation;" but, on the con-

* *British Medical Journal*, March 28, 1885.

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trary, much evidence exists, and will be referred to in the course of this essay, disproving the assumption of the existence of such a "virus" or of any other poison imported into the body from without, and demonstrating that the blood, except in so far as it is contaminated by bilious, urinary, or saccharine matter, owing to morbidly excessive action of the nervous system to be hereafter explained, is essentially healthy. Of course, if there is no trustworthy evidence of the existence of the alleged "chemical virus," the fact that it must be the product of "a living entity" is so wholly imaginary as to be quite outside the pale of scientific discussion.

But if the reality of this "chemical ferment" or "living entity" be assumed, we are left ignorant of its source, or how it is produced, and by what means it gains access to the human body; and though Dr. Klein affirms that "in some cases the linen and clothes are the means by which cholera is conveyed," and that "persons not sick of cholera may convey the disease from one part to another, which proves that the infective agency is not necessarily associated with the choleraic dejecta," he adds his testimony to that of hundreds of other witnesses that "*in hospitals devoted to the treatment of cholera the attendants remain unaffected as a rule.*" Moreover, he adduces a number of facts which, as he says, *prove that choleraic evacuations* "*are harmless.*" He adds: "From all my experience I conclude that the bodies of patients suffering from cholera do not contain organisms which could be regarded as specific and pathogenic, and in this I am directly opposed to Dr. Koch. In all true infectious diseases the organisms are present in the morbid products in great numbers; but such is not the case with cholera. When it is remembered that linen not soiled can convey the infection, and that water which had come in direct contact with choleraic discharges did not produce the disease, I consider that my position is rendered still more secure."

When this last-quoted passage and Dr. Klein's assertion that a "chemical virus" produced by a "living entity," outside the body, "acts on the blood, the nerve-centres, and the organs of circulation," and thus gives rise to all the phenomena of cholera, are considered together they prove rather bewildering, and I confess I find myself unable to harmonize them. In fact, it seems very difficult, if not impossible, to form any definite conception of Dr. Klein's "chemical ferment," which, having been produced by his "living entity," is, as he alleges, the cause of cholera; it is evidently very subtle and very capricious: while frequently renouncing the clothes of cholera patients, and even their evacuations, as well as "water which has come in direct contact with them," as means of conveyance from one person

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to another, it avails itself of "articles which have not been worn by cholera patients," and of "persons not sick of cholera," in order to "convey the disease from one part to another." Moreover, its period of "incubation" is so irregular as seemingly to defy the "reign of law:" it may be half an hour, or it may be several hours. But assuming that this "chemical ferment" has been generated, and that it has entered the human body by absorption or otherwise, the question arises,—How can it produce each of the several phenomena characteristic of cholera? I venture to affirm that it is impossible to imagine how it can do so; and certainly Dr. Klein has given no indication of how he supposes the result to be achieved.

Dr. George Johnson, in his paper already mentioned, simply re-stated the doctrine he has long held—viz.:

that cholera results "from the action of a specific contagium or morbid infecting agency, which may enter the system either with the air through the lungs, or with food, water, or other liquids through the alimentary canal." This "contagium enters the circulation, where it probably undergoes increase at the expense of some blood constituents, which are then excreted through the mucous surface of the alimentary canal, and are ultimately expelled by vomiting and purging, and thus the patient recovers." Dr. Johnson also maintains "that the main and essential cause of choleraic collapse is a greatly impeded circulation through the lungs;" that "an indiscriminate opiate and repressive treatment in the diarrhœal stage is believed to increase the danger of collapse by preventing or retarding the escape of the morbid poison;" that "the intestinal discharges are the means by which the disease is conveyed from the sick to the healthy;" and that "an evacuant or cleansing method is consistent with the true theory of the disease."

In my work "Diarrhœa and Cholera" I have submitted Dr. Johnson's views to an exhaustive criticism. I may, however, remark here that no evidence is adduced in proof of the existence of a "specific contagion," which is, therefore, as completely imaginary as is the "chemical ferment" of Dr. Klein; that its modes of entering the system, its "increase at the expense of some blood constituents," and its exit "through the mucous surface of the alimentary canal" are equally non-proven and imaginary. I shall hereafter show that the "greatly impeded circulation through the lungs" is one of the associated phenomena, and is certainly not, as Dr. Johnson alleges it to be, the cause, of collapse; and that the treatment which is the logical outcome of his theory—viz., purgation by means of castor oil—and which, if that theory be true, ought to prove pre-eminently successful, is, in fact, proved by experience both in England and in India to be more appallingly fatal than almost any other which has been tried.

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It will probably be admitted that, after listening to the discussion here referred to, Sir Joseph Fayrer was justified in stating that, in his opinion, it has not brought us "one iota nearer the truth as to the *causa causans* of cholera" than we were before.

The doctrine of Dr. Klein, and that of Dr. George Johnson just described, are not disparaged because they are hypothetical, but because they afford no real explanation of the phenomena of cholera. The advancement of science, and especially that part of it comprising physiology and pathology, is due in large measure to the origination of comprehensive hypotheses. As justly observed by Sir Andrew Clark: "Hypotheses are at once the effects and causes of progress; one might as well attempt to preserve and employ an army without organization as to preserve and employ phenomena without a theory to weld them into one. But the theory must be provisionally, if not positively, true; it must be intelligible and consistent; it must explain a greater number of facts, and reconcile a greater variety of apparent contradictions, than any which has preceded it; and it must have become developed not by the addition merely, but the addition and solution, of subsidiary explanations." Now, I am encouraged to believe that the hypothesis concerning cholera described and advocated in the following pages fulfils the conditions expressed by Sir Andrew Clark. Indeed, he says as much. Referring to this hypothesis in his review of my book "Diarrhœa and Cholera," from which the above passage is quoted, he remarks:

"It is in harmony with the results of the most recent physiological investigations. . . . . It is only by a close examination of the detailed application of the hypothesis as a means of rendering intelligible the proximate cause of every special symptom that a comprehensive conception of the hypothesis becomes possible. Each symptom receives a consistent and intelligible explanation. . . . . The strength of the theory lies in its comprehensive and simple explanation of seemingly contradictory phenomena by the application of a recognized general truth." *

This judgment, pronounced nearly twenty years ago, has since received striking confirmation in two different forms. Certain important phenomena the causes of which I had not then adverted to—viz., the presence of sugar in the urine, the typhoid symptoms, the greater mortality of males than of females, and the especially great abundance of comma bacilli in the intestines—are now for the first time explained; while, on the other hand, the correctness of the theory has received additional verification from

* *Medical Times and Gazette*, Nov. 3, 1866.

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experience of the successful treatment of the disease according to the method which that theory indicates.

The hypothesis in question negatives the doctrine that cholera is contagious. As this panic-spreading doctrine is baneful in the extreme, and as I believe myself able to present proofs, both implicit and explicit, that it is baseless—that, in fact, cholera is in no sense of the word contagious, and that it is indubitably and rapidly curable, it seems to me especially expedient at the present time that I should lay my conclusions before the public, together with an exposition of the facts and arguments on which they are founded. Compared with the magnitude of the subject, that exposition will necessarily be extremely brief; and, inasmuch as many physiological and pathological facts which it would mention in detail, were it addressed to medical men, must here be either ignored or merely referred to, it will of course be very defective. I hope, however, by using language as simple and untechnical as I can command, to render the argument I am about to expound easily apprehensible by the general reader.

SECTION II.

The Nervous System: Cerebro-spinal and Sympathetic.

In man, as in all the higher animals, the nervous system consists of two great divisions—viz., the Cerebro-spinal and the Sympathetic. The former comprises the brain and spinal cord, together with the forty-three pairs of nerves which are given off from them. It is by the intervention of cerebro-spinal nerves that our voluntary muscles are brought into action, that the various glands and glandlets throughout the body are made to secrete, and that the manifold impressions of all kinds which are made on the peripheral ends of our sensory nerves are conveyed to the nervous centres—the spinal cord and brain. The sympathetic nervous system consists, chiefly, of a series of little masses of nervous matter called ganglia (knots), arranged along each side of the spinal column, and connected by intermediate nerve filaments so as to form two knotted cords. These extend from the upper part of the neck along the spinal column to the lower extremity of the sacrum called the "coccyx," in front of which the cords of the opposite sides are united in a single ganglion. This

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ganglionic system communicates with every pair of cerebro-spinal nerves as they emerge from the spinal cord, and also unites with cerebro-spinal nerves to form numerous "plexuses," from which branches are distributed to all the organs contained in the thoracic, abdominal, and pelvic cavities.

A striking and especially interesting feature in the distribution of the force emanating from the Sympathetic is the great amount of it which is supplied to the heart and blood-vessels (chiefly arterial) throughout the body. Delicate clinging filaments from the sympathetic ganglia surround the great arterial stem, the aorta, originating from the left heart, and all the branches, terminating in the minutest twigs, from that stem, in much the same manner as ivy twines itself around the stems and branches of trees. The functions of this remarkable assemblage of ganglionic nerve-centres long remained a mystery, and, in some respects, remains a mystery still. But, happily, its chief function, at all events, has been revealed: were it not for this revelation the essential nature of cholera must have continued unknown and unknowable.

The great and lasting honour of discovering and of demonstrating the main function of the Sympathetic was reserved for that profoundly sagacious and pre-eminent physiologist, the late Professor of Physiology at the Collége de France, Claude Bernard. Every artery is surrounded by a series of muscular rings so intimately joined together as to form a continuous muscular tube—called the muscular coat—which formerly was supposed by its elasticity to aid in propelling onwards the successive waves of blood originated by the heart. In 1851 Bernard proved that the involuntary muscles constituting the muscular coat of arteries are as thoroughly dominated by, and subservient to, nerve-force as are the muscles which are subject to mental control (those of our limbs, for example) and which execute our volitions; and that the nerve-force which governs these involuntary muscles emanates from Sympathetic nerve-centres, and is distributed to those muscles by Sympathetic nerves. When endeavouring to find out in what way the nervous system contributes to the generation of animal heat, Bernard divided the Sympathetic, or ganglionic, cord in the neck of a living rabbit. Immediately afterwards, on the side on which the nerve had been divided, the blood-vessels of the conjunctiva (the mucous membrane covering the front of the eye and lining the eyelids) and of the ear became visibly and greatly distended; the temperature of the face and head on the same side rose *seven degrees Centigrade* (about 12° F.) above that of the sound side; sensibility was increased; and, generally, the phenomena of an increase of vital action presented themselves.

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In 1852, Brown-Séquard, the distinguished compeer of Bernard, and now his successor as Professor of Physiology at the Collége de France, first, and afterwards Bernard and the English physician, Dr. Augustus Waller, galvanized the upper part of the severed ganglionic cord, and found that the effects produced by doing so were the reverse of those consequent on its section: the blood-vessels contracted; the quantity of blood supplied by them was lessened; the temperature was lessened; and, generally, the phenomena of a decrease of vital action presented themselves. In fact, galvanization of the Sympathetic can produce contractions so vigorous of the blood-vessels subject to the part of the nerve galvanized as to suspend or arrest almost wholly the circulation of the blood in them. Owing to its wonderful power of contracting arteries, this nerve can render the parts supplied by those it constringes pale and bloodless; it can retard or arrest textural nutrition, and, therefore, the organic functions; and (as by arresting textural nutrition, it arrests those chemical processes which form an essential part of the changes associated with that nutrition, and in consequence of which heat is evolved) it can cool the body to the extent even of rendering it nearly as cold as a corpse. Considering the last-named results of the action of the Sympathetic, Bernard named it the *Frigorific* nerve.

The foregoing very imperfect, because necessarily very brief, explanation in respect to the nervous system will, I hope, enable my readers to understand without difficulty the following exposition of the essential nature of cholera.

SECTION III.

The Proximate Cause, or Essential Nature, of Cholera.

It is often observed that our first impressions of persons with whom we become acquainted are the truest, and scarcely less often experience justifies the observation: when English physicians first became acquainted with cholera in India, where it presents itself in its most terrible aspects, and before they had formulated any theories concerning it, they were most deeply impressed by its spasmodic character; and hence, regarding the nervous system as primarily affected, they designated the disease *cholera spasmodica*. Since then pathologists have made a wide circuit, searching in every other constituent of the body for the

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essential nature of the malady; but they have not found it, and I shall endeavour to show that those first impressions of Anglo-Indian physicians, which originated the name *cholera spasmodica*, were correct—that, in fact, cholera is essentially a disease of the nervous system.

During the last half-century many students of cholera have ascribed it to disorder of the nervous system. In 1831, Dr. G. H. Bell, in his work "Cholera Asphyxia," endeavoured to prove that the disease is due to a morbid state of the Sympathetic. Several other Anglo-Indian physicians, impressed especially by the spasmodic phenomena of the disease, ascribed it, as already mentioned, to disorder of the nervous system. In 1832 a French physician, Dr. L. Auzoux, published the doctrine "that cholera is to the great Sympathetic what epilepsy is to the brain." Dr. Davey, in his work on the "Ganglionic Nervous System," published in 1858, claims to have shown that cholera is due to a morbid state of that system. The eminent English physician, Sir William Gull, refers the symptoms of the disease to "an early and severe depression of the ganglionic nervous centres; Dr. Copland, in his "Medical Dictionary," expresses a like opinion; and Dr. Goodeve, one of the most recent of the English authoritative writers on cholera, says that the state of the lungs and intestines implies that the nervous system is under a morbid influence. But though the authorities in favour of the hypothesis that, to a large extent at least, cholera is a disorder of the nervous system, are numerous and weighty, all of them ascribe the disorder exclusively to the action of the Sympathetic: the *rôle* of the cerebro-spinal system is wholly ignored, and while several of these pathologists are of opinion that the sympathetic nervous system is profoundly depressed or exhausted, no one of them explains in what consist the several links in the chain of causation between the alleged disorder of this nervous system and the manifold symptoms of the disease.

The doctrine which I believe affords a complete and consistent explanation of all the seemingly mysterious phenomena of cholera may be thus expressed: *All the symptoms of cholera are due to simultaneous and abnormal superabundance of blood in, and excessively preternatural activity of, both the spinal cord and the sympathetic nervous centres.*

In my work on "Diarrhœa and Cholera,"* at p. 100, I have enumerated thirty-eight characteristic symptoms of the disease; and of these twenty-one are, as I have shown, produced by morbidly excessive activity of the spinal cord. In the early

* "Diarrhœa and Cholera: their Nature, Origin, and Treatment through the Ageney of the Nervous System." London, 1866.

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stages of the disease *all* the secreting organs are preternaturally active: the renal, hepatic, pancreatic, mucous (including the intestinal), and sweat glands are pouring out their appropriate secretions superabundantly. Even the serous membranes are secreting more copiously than is their wont.* Now, all these functions are carried on by virtue of force conveyed, there are valid reasons for believing, through spinal nerves, from the spinal cord. The question—What is the immediate cause of the immense outpour from the stomach and bowels? is at once so important and so interesting that I shall venture to advert to it particularly. It is remarkable, as already intimated, that nearly every writer on cholera, when adverting to the state of the nervous centres related to the intestines, speaks of them as being profoundly depressed or exhausted, and considers that the proper functions of secretion are arrested. It is probable that the large amount of serous or watery effusion which occurs has concentrated attention upon itself, and, to the eyes of the majority of observers, has masked the important fact that the actual secretion of mucus is enormously increased. The large amount of whitish flocculent matter constituting the characteristic element of the so-called rice-water evacuations has, however, been the subject of elaborate and prolonged researches and discussions, and has given rise to a great diversity of opinion as to its essential nature. In my work just mentioned, I have reviewed the evidence adduced on this question, and have, I believe, demonstrated decisively that, so far from the nervous centres related to the intestines being, as they have been generally said to be, profoundly depressed or exhausted, they are, in fact, in a state of the most extreme and tumultuous, activity. I must, however, content myself here by adducing but one witness in support of this statement; and his evidence may perhaps be held to be of special value, not only because he has been a careful observer of the nature of the choleraic discharges, but also because, having put forth a theory of cholera which differs widely from mine, he will probably be regarded as an impartial witness: I refer to Dr. George Johnson, who says,—

"The flocculi in the rice-water stools consist almost entirely of *perfectly organized epithelial cells, most of them of large size.* Of this

* Before I became aware that this fact had been actually observed, I foresaw it by the light of the hypothesis here expounded. Having had no *physical* demonstration of the fact, and having seen no record of it in any work on cholera, I was greatly disconcerted, inasmuch as the hypothesis predicted and required the phenomenon. After a few weeks of discomfort and anxiety on this account, I was immensely relieved by finding a record of the observation of the phenomenon in the work on cholera which was published by Dr. Shrimpton, and of which he was good enough to send me a copy. He states that he observed the fact during post-mortem examinations made by himself.

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fact I have satisfied myself by repeated examinations of the discharges from different patients. The peculiar creamy viscid secretion, which sometimes nearly fills the small intestines after death, is almost entirely made up of the same *fully formed epithelium*. Now, it is obvious that this large amount of epithelium cannot be explained by the peeling away of one or two layers of cells from the surface of the mucous membranes—the result of a local irritation during life, or of maceration by the fluid contents of the bowel after death. *This abundant cell-formation can result only from a very active vital effort.*"

In presence of the large array of facts pertaining to this question now accumulated, it is impossible to avoid the conclusion, that the whole glandular system of the alimentary canal is in a state of the most energetic, tumultuous activity; that the development of gland cells, which are shed in quick succession, is extraordinarily exuberant; and that a very large proportion at least of the cells, and cell-detritus, as well as the whole of the superabundant mucus found in the evacuations, and in the intestines after death, is the product of this excessive development from the vast extent of the glands and glandular surface in question. A recognition that the functions of this large body of secreting cells are thus enormously augmented and intensified, produces the conviction that the nervous influence distributed to the entire glandular surface must have been overwhelmingly energetic and intense; and this conviction becomes a certainty when we consider the thoroughly ascertained fact that during the period of collapse, or the algide phase, of cholera, while the temperature of the surface of the body is lower, that within the part of the bowels to which the thermometer can be applied—viz., the rectum—is higher than normal. The temperature in the rectum is not infrequently 102° F., and in some cases exceeds, even, 105° F. During the epidemic of cholera in London in 1866, Mr. F. M. Mackenzie made "Notes on various subjects connected with cholera as observed in the London Hospital," at which he was then assistant resident medical officer. These Notes* contain a statement of the temperature which he observed in the armpit and in the rectum, or vagina, respectively, in forty cases of "extreme collapse." The average temperature of these cases was 94·9° F. in the armpit, and 100·6° in the vagina or rectum. The extremes of temperature noted were 91·2° and 105·6° F. In instructive accord with these facts, the temperature in the groin is found to be about 2° F. higher than it is in the armpit. My discovery that the mucous membranes generally may have their functional activity increased by the application of heat

* These valuable Notes are contained in the Appendix to vol. iii. of the "Clinical Lectures and Reports by the Medical and Surgical Staff of the London Hospital." London, 1866.

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along the spine, and lessened by cold along the same region,* while revealing how it is that, simultaneously with general spasmodic contraction of the arterial system, such immense outpourings from the mucous glands of the alimentary canal are possible and are effected, prove conclusively that preternatural functional activity of the spinal cord is at once the proximate cause and complete explanation of this hitherto mysterious phenomenon. To the excessive action of the nerves presiding over the function of the kidneys is also due the hyperæmia or congestion of these organs which is the immediate cause of the phenomenon often observable in cases of cholera—viz., the presence of albumen in the urine.

Another important group of symptoms consists of morbid conditions of the muscular system. These are—abdominal griping; excessive contractions, or expulsive activity, of the stomach and bowels; excessive activity of the thoracic and abdominal muscles; excessive contraction of the urinary bladder; tremors; muscular twitchings; fixed and stony expression of the face; tonic hardness of some, or of many, of the voluntary muscles; tightness across the lower part of the chest; cramps and convulsions. Now all these morbid phenomena of the muscular system are exclusively due to over-activity of the spinal cord, which discharges its tumultuous excess of nerve-force through the spinal-motor nerves, on the muscles with which they are connected. The whole of the morbid conditions already mentioned consist of an excess of function of the organs implicated. Hence, for convenience of description I call them *positive* phenomena; and the nerves which are concerned in producing them I call *positive* motor nerves.

On the other hand, there is a number of symptoms which are exclusively due to the excessive activity of the Sympathetic, and the character of which is accurately denoted by the word *deprivation*. All the symptoms in question are due to the fact that the affected organs are deprived of their wonted supply of blood. Hence I call this group of symptoms *negative*, and the Sympathetic which produces them the *negative* motor nerve The first group of these symptoms is cerebral—viz., slight headache, deafness of various grades, singing in the ears, dizziness, slight faintness, syncope, enfeeblement (but without perversion) of the mental faculties. These conditions are all clearly referable to the state of cerebral anæmia induced by the undue contraction of the brain-arteries caused by the constricting action of the Sympathetic.

* For evidence of the truth of this statement, the reader is referred to the introductory chapters in my works on "Sea-sickness" and "Diarrhœa and Cholera" respectively.

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As the disease advances, the arteries which supply the lachrymal and salivary glands, the liver and kidneys, become so powerfully constricted that blood ceases to flow to those organs, and hence their appropriate secretions become fearfully "conspicuous by their absence." The negative character of the pulmonary group of symptoms is not less striking. The involuntary circular muscles, not only of the pulmonary blood-vessels, but of the bronchial tubes, are powerfully constringed. Hence the impediment to the pulmonary circulation; the distension of the right heart and pulmonary artery, and comparative emptiness of the left heart observable after death; the arrest, more or less complete, of the normal chemical changes which take place in the lungs; the shrinking of the lungs themselves, and its obvious consequences—short, struggling, and rapid respiration, enfeeblement of the voice, voicelessness, cold breath—devoid, or all but devoid, of carbonic acid; and in a large proportion of cases the presence of sugar in the urine. Gradually the arteries of the skin become so contracted that the blood is shut off from the surface of the body, the result being the well-known and extremely characteristic group of symptoms called "Algide"—symptoms which include the shrunken, death-like aspect of the visage, the corrugated condition, dark, or blue colour (cyanosis), all but abolished sensibility, and corpse-like coldness of the skin. The fatally constringing energy of the violently excited Sympathetic finishes its work by depriving, first the voluntary, and then the involuntary muscles of their needful supply of blood—death consequently overtaking each in the same order of succession. And though the brain still lives, the patient, while possessing consciousness and more or less intellectual capacity, including the power of hearing and understanding what is going on around him, may yet be unable to give any sign that such is the case.

Finally, death overtakes the nervous system itself. As correctly stated by Professor Parkes: "two or three hours before death there is often some return of heat in the scalp and forehead, over the region of the heart or whole chest, and it may be also over the abdomen; the extremities are still icy cold, and the cholera visage is unaltered. This partial return of heat on the head and trunk is an immediate forerunner of death, and, as far as I have seen, is invariably a fatal sign; it is occasionally confined altogether to the cardiac region, and is sometimes astonishingly great." Mr. Mackenzie reports that "a great many" patients who died and in whom no reaction was observed, "had an elevation of temperature externally, as well as internally, while dying and after death." This remarkable phenomenon is immediately due to the relaxation of the various branches of the

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pulmonary artery, of the bronchial tubes, and of those systemic arteries distributed over those parts of the body which, during the disease, have continued most vital, and have therefore preserved the highest temperature. The blood has continued to pass through these arteries most copiously, and is thus prepared to effect their dilatation, and to flow through them in fuller currents than before, the moment the energetic stimulus from the negative motor nerves, which has kept them in a state of tonic spasm, declines or ceases. Now already, when the phenomenon in question presents itself, the ganglionic nervous centres presiding over the arteries just mentioned have begun to die. Their convulsive grasp of the blood-vessels and air-tubes, which has already proved fatal to the system generally, is being relaxed, and they themselves are sharing the fate which, through their agency, has overtaken the entire organism. A last but vain effort for life is made, however, by the structures, released at length from the deadly influence of their excessive energy. The normal attraction between the venous blood in the pulmonary arteries and the air in the air-cells generates movement of the blood through the pulmonary capillaries; it reaches the left heart, and is thence forced most copiously into those systemic arteries just indicated, and thence, finding its way to the starving tissues, the usual vital changes occur. Meanwhile, as a result of the renewed oxydation occurring in the lungs, and in the parts supplied by the newly relaxed systemic arteries, heat is evolved, and constitutes the phenomenon in question. But this local struggle for life is too late: its possibility depends on the presence of death which has already invaded the nervous system, and which, soon seizing on the brain itself, closes the scene. This local increase of temperature before death is a strong proof that the blood-vessels are healthy; that the structures are healthy, and suffer only from lack of nourishment; that the blood itself is free from poison; and that the disease is seated in the nervous system.

The increase or long persistence of heat in the body after death, is a well-established fact. "The highest temperature I have taken after death," says Mr. Mackenzie, "was in a little boy; externally, it rose to 101·8°; internally, to 106·2°; this boy died in imperfect reaction. It is rare to find the temperature as high as this; usually it will rise to 99° in axilla, and 102° in rectum." This remarkable and very instructive phenomenon is merely the continuance and extension over the whole body, after the death of the brain, of the series of actions commenced before death, and explained in the preceding paragraph. The whole sympathetic system having ceased its functions, the arteries throughout the body relax; the small supply of blood in them is drawn through the systemic capillaries; every particle of oxygen

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which it can yield up combines with the surrounding structures; and while there are elements to continue these changes, the temperature of the body is raised, maintained, or prevented from declining with the rapidity usual after death from almost all other diseases. The reflux of blood throughout the body denoted by the general increase or unusual persistence of its heat must inevitably result in lessening the bulk of any organ which may have been unduly distended with blood. Such an organ in choleraic collapse is the spleen, which is found unusually large during life, but which after death presents in respect to size no constant appearance. It seems to me probable that when the evacuations are extremely abundant, the spleen is less distended than in other cases, and that the great variations of size which it presents after death depend upon the amount of the evacuations, and the extent to which the post-mortem arterial relaxations result in a temporary renewal of textural vitality, which, of course, implies to the same extent a diffusion of the previously pent-up blood throughout the body.

Those post-mortem changes in the aspect of the skin, which in cases of cholera are especially notable, result from the chemical changes occurring, and caused as described in the preceding paragraph. The skin becomes lighter in colour, even when it has been especially dark, and has continued so for some time, because the oxydation of the blood changes it from blue to red, while the relaxation and partial dilatation of the terminal arteries cause the shrivelled aspect of the face, hands, and feet, partially or wholly, to disappear.

Those extraordinary and, to the ignorant, terrifying phenomena — post-mortem muscular contractions, have never, I believe, received a satisfactory explanation; but the hypothesis here expounded reveals the cause of them at once. The arteries supplying the muscles are vehemently contracted before death, but after death they are relaxed; immediately before death the muscles are deprived of blood, and therefore enfeebled to the utmost degree; after death, receiving a new supply of blood, their hungering constituent elements receive fresh nourishment, with it new strength, and, *under the stimulus of the still hyperæmic spinal cord, which is the last to die,* contract in the manner described. It seems to me that if the waning life of the blood in the spinal cord be equally distributed, those apparently *co-ordinated* muscular movements sometimes observable are most likely to occur, and that, when special segments of the cord retain their excitability longer than the rest, convulsive movements of one or of a few muscles only will result.

The early onset of rigor-mortis is a fact characteristic of cholera corpses, and is in extremely interesting accordance with

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the requirements of the hypothesis now expounded. When the Sympathetic of an animal is divided, and one part of it is excessively stimulated by means of galvanism, the arteries to which that part is distributed become, of course, strongly contracted, and in this manner a condition like to that which I affirm to exist in the arteries of a cholera patient in collapse is induced. Now, in such cases, rigor-mortis invariably supervenes in the part of the animal thus experimented upon far more rapidly than elsewhere; and the fact that the same condition comes on with extraordinary rapidity after death from choleraic collapse is a striking proof that the arteries of patients in that state are powerfully constringed by nervous agency.

Additional evidence in support of the hypothesis in question is derived from the post-mortem appearances in cases of death during collapse. The distribution of the blood generally is always in the veins. "Arborescent venous congestion" is the prevailing epithet used to describe the aspect of the vascular system, the arteries being empty. This is the condition which the hypothesis presupposes. Whatever may be the amount of hyperæmia of the spinal cord and ganglionic nervous centres during life, it by no means follows that they will be found in the same condition after death. Indeed, what has already been stated concerning the change in them after death, and its results in the arterial system, proves that their vascular state is modified when death occurs. It is, however, said that the spinal cord is found extremely congested, and that the sympathetic centres examined by Mayer were disorganized. These statements need confirmation. But if the sympathetic ganglia were disorganized, the fact would imply that their functional activity had been intensified to the extent of inducing destructive inflammation, and, if so, this is the strongest fact which could be adduced in proof of the doctrine here propounded.

This brief exposition of the proximate cause of cholera may be fitly closed by a reference to the state of the blood of patients dying in collapse. It has been the object of a large amount of careful investigation by numerous observers, who, on the whole, concur in the conclusion that whatever differences in its constitution from that of normal blood are discoverable are differences mainly due to the withdrawal from it of a large amount of its most fluid portion. There is no evidence that the blood is in any other respects unhealthy. The truth of this remark is strikingly confirmed by the experiments of Professor Parkes: he proved not only that by inflation of the lungs after death the dark blood in them becomes vividly red, but that when it is taken out of the body and exposed to the air in thin layers it does the same. "It is certainly a very singular thing," he says,

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"that in cholera the blood should retain its power, out of the body, of acquiring a red colour when exposed to the air in thin layers, and yet, in the body, that this change, as well as the changes leading to the production of heat, should be interrupted. *This certainly looks like obstruction only.*" These words which I have put in italics show how near the truth of the matter Professor Parkes stood. "It has been affirmed," says Professor Bennett, "that the colour and number of the corpuscles of the blood undergo a change in cholera, but exact observations are wanted to confirm the statement. I have never been able to satisfy myself that any such changes were observable by means of the microscope."* In fact, though the morbid processes to which the phenomena of cholera are due are usually supposed to consist of chemical changes wrought in the blood by an organic poison, all that we know of the state of that fluid tends to prove that it is essentially healthy, that the changes effected in it are merely changes in the relative amount of its constituents, and that those changes are consequences of purely physical or dynamic agencies.

There is, however, in many cases of cholera an apparent exception to the truth of the general statement just made—viz., that the blood "is essentially healthy;" for, as stated at p. 15, the urine is in a large proportion of cases saccharine; and, of course, saccharine urine denotes the presence of sugar in the blood from which it is excreted. But sugar in the blood cannot be regarded as a "blood-poison" in the ordinary sense of that term; sugar is a normal constituent of the blood in the hepatic vein and right heart, and *traces* of it in arterial blood are not inconsistent with perfect health; moreover, its presence in cases of cholera is by no means an essential or invariable symptom of the disease—is only observable probably in about half the cases—and, when present, is evidence merely that the morbidly excessive nervous force which is being exerted acts, in these cases, on the lungs in an especial degree. The significance of sugar in the urine of choleraic patients as evidence of the existence of disorder of the nervous system is especially important. The question—What is the immediate cause of glycosuria in cases of cholera? is peculiarly interesting; and, as I have not adverted to it in "Diarrhœa and Cholera," I will endeavour to answer it here. Of the twelve cases of cholera hereafter mentioned, which I treated at Paris in 1884, there were seven in which the urine was saccharine. The quantity of sugar in it was generally small: M. Berthoux, "Interne en pharmacie" at l'Hôpital de la Charité,

* "Clinical Lectures on the Principles and Practice of Medicine." By John Hughes Bennett, M.D., F.R.S.E. Fifth Edition. P. 93.

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examined the urine in these cases, and found that the quantity differed in the different cases from about two to six grammes per litre.

The chemical transformation of the amyloid or starch-like substance which exists in the liver into sugar, followed by its presence in the blood, and therefore in the urine, may be induced by a great variety of causes. It has been ascertained that obstruction to the breathing, and other forms of obstruction to the proper flow of blood through the system, will, as stated by Dr. Pavy, "occasion a highly saccharine state of the blood. . . . . For example, it has been observed in whooping-cough, coma, and pneumonia." He refers the diabetes of pneumonia and coma to congestion of the liver, and that of whooping-cough to congestion and the violent action of the abdominal muscles. Dr. Pavy's explanation of the presence of diabetes in these diseases may be correct, but, if it is, it throws no light on the subject in question, for during the algide stage of cholera the liver is not congested, but anæmic. According to the evidence of post-mortem examinations, its volume is lessened; it is paler than it is in its normal state, and it is bloodless.

In healthy persons the blood which passes from the liver to the lungs is throughout the whole course of its passage saccharine. At its exit from the liver through the hepatic vein it contains a maximum amount of sugar; after it mixes with the blood flowing from the lower extremities, the pelvic and the abdominal viscera, it is, of course, less saccharine; and as these conjoined currents (in the vena cava ascendens) mix in the upper chamber of the right heart (the right auricle) with the blood flowing into it (through the vena cava descendens) from the head and upper extremities, the blood from the liver is diffused throughout that brought from all parts of the body. Hence in its passage from the liver to the lungs the sugar is least manifest in the blood of the right heart. While this blood passes through the lungs from the right to the left heart it loses its saccharine character; during its transformation from venous into arterial blood the sugar in it is also chemically transformed—probably by oxydation—so that when the blood converging from the lungs reaches the left heart it contains, as a general rule, no trace of sugar. It is therefore evident that an important function of the lungs consists in the chemical transformation and consequent destruction of the sugar which exists as a normal constituent of the blood-current between the liver and the lungs, the arterial blood being thus effectively protected from invasion by the saccharine element. It can only be thus protected, however, when, as in healthy persons, the quantity of sugar carried from the liver is small; when that organ produces it in excessive

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abundance it cannot be destroyed as fast as it reaches the lungs, and hence the disease—diabetes mellitus. Now, as stated above, the quantity of sugar in the urine of cholera patients is usually small—so small as to be easily disposed of in the lungs were they in their normal state; therefore, the presence of sugar in the urine in these cases affords no reason for believing that the liver is producing a morbidly excessive amount of sugar as it does in ordinary diabetes mellitus.

But during the algide stage of cholera the lungs are extremely contracted, so that in those cases in which the disease is most characteristically developed the patient is voiceless and his breath is cold. Post-mortem examinations reveal to us that the lungs are remarkably contracted, flaccid, and bloodless, except in the lower and posterior parts, which, generally, are found to be congested. There is a remarkable diminution in the weight of the lungs, which are from 10 to 20 ounces lighter than the healthy lungs of persons dying from other diseases. The condition of the lungs in cholera cases being what it is, their functional activity is reduced to, or nearly to, the minimum compatible with the continuance of life, and, consequently, those chemical changes of the blood effected in normal lungs take place to only a very slight degree, and quite insufficiently to oxydize or otherwise destroy the sugar which is carried to them in the venous blood, and which, therefore, passes in the lessened and sluggish current to the left side of the heart. Thus it is, I venture to affirm, that the blood in the arteries contains sugar, and thus it is, therefore, that the urine of patients in choleraic collapse becomes saccharine.

But though the immediate cause of the presence of sugar in the urine of cholera patients consists in the arrest, more or less complete, of the pulmonary functions, the cause of that arrest exists in the nervous system. As already explained, the excessively energetic action of the sympathetic nerve results in the contraction of all the muscular tissue subject to its control, and, therefore, of the muscular coat of the bronchial tubes as well as of the systemic and pulmonary arteries. Hence it is that while the contractions of the pulmonary arteries impede the access of their contents to the pulmonary capillaries, the contractions of the bronchial tubes impede the access of air to the air-cells. This doctrine harmonizes perfectly with all the pulmonary symptoms of the disease, is the only one which explains them, and is, I believe, the only one which explains the temporary presence of sugar in the urine of cholera patients.

But, whether or not the explanation of the presence of sugar in the urine of cholera patients here given be the true one, it will be generally admitted, at least by those most competent to

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express an opinion on the subject, that in the great majority of cases of saccharine urine the disease originates primarily in some lesion or disorder of the nervous system, and, therefore, that the presence of sugar in the urine of a person suffering from cholera is strong evidence, so far as this symptom is concerned, that his nervous system is morbidly affected. Indeed, from any point of view, this very interesting phenomenon and its causal relation to the nervous system constitute an additional and important link in the chain of argument by which my hypothesis concerning the essential nature of cholera is sustained.

As the presence of sugar in the urine is due to the excessive action of the Sympathetic or "negative" motor nerve-centres controlling the blood-vessels of the lungs, so two other important phenomena, together with their too often serious consequences, to which I invite especial attention—viz., the suppression of bile and urine—are due to a like cause. Owing to the powerfully constringing influence of the sympathetic nerve-centres which innervate the blood-vessels of the liver and kidneys, those vessels are gradually contracted and in many cases finally closed. Thus, in the manner now described, owing to the comparative inaction of the lungs, sugar which would otherwise be got rid of, is retained in the blood; and the liver and kidneys, being deprived of blood, are prevented from secreting. Therefore, the biliary and urinary constituents, which those organs when duly fulfilling their depurating office separate from the blood remain in it, and, together with the sugar which it also retains, exert their poisonous influence on the whole organism, the brain being thus in an especial degree injuriously affected. Hence the source and cause of the so-called "typhoid state" which, in a considerable proportion of cases, is observable during the reactionary stage of cholera, and which is very frequently the forerunner of death.*

Now, if the algide stage is especially prolonged, the constringing energy of the Sympathetic, distributed to the bronchial tubes and to the blood-vessels of the lungs, liver, and kidneys, is very slowly spent, and the retention in the blood of sugar, bile, and the distinctive elements of the urine continues a proportionately long time; these substances, accumulating in the organism, poison the blood to a degree proportionate to the intensity and duration of the algide state, and the typhoid symptoms become correspondingly prominent. The frequent predominance

* According to Dr. J. Macpherson, during a period of ten years 20 per cent. of the fatal cases in the Calcutta General Hospital were lost in the stage of reaction; and of the total mortality in the Pharo Hospital at Marseilles, in 1884, the proportion during the same stage was 28 per cent.

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of these symptoms, and the frequently fatal termination of cases during the stage of reaction (when the process of poisoning just described has long continued), have been adduced as evidence that cholera is a result of a specific poison infecting its victims in some mysterious way which no one hitherto has been able to explain. Accepting the hypothesis I have propounded concerning the proximate cause of cholera, the reader perceives at once that the blood-vessels *must* become constricted, and in many cases closed; that sugar *must* be retained in the blood; that the bile and urine must become scanty or suppressed; that in proportion to the degree and duration of the scantiness or suppression the blood *must* become poisoned by these substances thus retained in it; and that such poisoning *must* induce typhoid conditions varying in intensity in correspondence with the extent of the poisoning. It is clear, therefore, that the typhoid phenomena of cholera are inevitable products of the excessive energy of the "negative" motor nerve-centres which innervate the structures in question, and, in reality, lend no countenance whatever to the theory that cholera is originated by a poison imported into the organism from without.

The doctrine here expressed respecting these phenomena is impressively verified by the experience gained in treating cases by the method I have introduced: owing to the action of the spinal ice-bag in relaxing (*inter alia*) the structures just mentioned in cases thus treated, these abnormal contractions last so short a time that the poisoning in question occurs to so slight a degree as to be scarcely appreciable, and hence, when reaction supervenes on the algide condition, no typhoid symptoms ensue. Of the twelve cases treated by me at l'Hôpital de la Charité only one exhibited typhoid symptoms; and that one (case 3) was the case of a woman who entered the hospital November 9 (she was the first cholera patient admitted), five days before I went there for the first time. Not having had the advantage of the treatment in question while she was still in the algide stage, she was "somnolent" when I first saw her on the 14th; she afterwards manifested symptoms of low delirium, and recovered so slowly that she was the last, or the last but one, of the cholera patients to leave the hospital.

I will close this section by mentioning a curious and instructive fact referred to as yet only once, so far as I know, in the literature of cholera—a fact the chemical significance of which remains, I believe, unknown, although its pathological significance is unmistakable:—In several of the cases of cholera at l'Hôpital de la Charité in 1884, the urine, to which nitric acid was added, assumed a beautiful colour, suggestive of a mixture of the colour of the violet with that of the ruby. This fact had

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already been observed at the London Hospital in 1866, and is thus recorded in the Report (p. 451) for that year:—"With nitric acid the urine, in many cases, gave a brilliant ruby colour, *resembling that often seen under the same reaction in nervous disease.*" It thus appears that this remarkable fact is a striking addition to the proofs already given that cholera is a "nervous disease."

So far as is practicable within the compass of this essay, I have now shown that all the symptoms of cholera are produced by a preternatural, tumultuous energy and activity of the nervous system—both cerebro-spinal and sympathetic; and that, though extremely numerous and various, those symptoms are completely accounted for by the operation of one and the same immediate cause. In concluding this part of the subject, I may observe that while the hypothesis now briefly expounded does not exclude the possibility of the existence of a cholera poison, germ, or microbe, it is self-sufficing, and that this fact renders it extremely improbable either that any such poison, germ, or microbe is causative of cholera, or that the disease is in any sense more infectious or contagious than is sunstroke or epilepsy. Sir Andrew Clark, in his critical analysis of my work, "Diarrhœa and Cholera," remarked that I might happily have prefixed to it for motto the axiom of Sir Isaac Newton: "Causas rerum naturalium non plures admitti debere, quam quæ et veræ sint et earum phænomenis explicandis sufficiant:" now the nervous system in the state described above affords a sufficient and complete explanation of the production of the phenomena in question; therefore, to recognize even the possibility of a blood-poison as the immediate cause of cholera would be, according to Newton, not only needless, but contrary to the dictates of a sound scientific method.

SECTION IV.

The Remote, Predisposing, and Exciting Causes of Cholera.

The phenomena of cholera are ascribable to proximate, to remote or predisposing, and to exciting causes. The term, *proximate cause*, is generally understood to denote that morbid condition of any given structure the presence of which entails the symptoms characteristic of the disease in question as an inevitable consequence. This immediate cause is

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therefore justly regarded as the essence of the disease; and hence the determination and description of its seat and character constitute pathology. The pathology of cholera having been already discussed, I have only to advert here to its *remote* or *predisposing* and *exciting* causes. These terms are, in my opinion, objectionable; for, in respect to the causes of cholera, at least, they are often interchangeable: thus, if the pathology of cholera explained above be correct, atmospheric heat may induce hyperæmia of the nervous centres, and so predispose them to excessive functional activity. If now the person in whom this change has occurred drinks freely of alcoholic liquors, or water containing a considerable proportion of organic matter, he may directly, through the stimulating influence of the alcohol on the nervous centres, or, indirectly, through irritation of the nerves of the alimentary mucous membrane by the bad water, so excite those centres as to produce cholera; and, conversely, alcohol or bad water acting as stated on nervous centres not already predisposed to cholera, may render them hyperæmic, thus inducing in them a predisposition to the disease, and, when this condition has been established in this manner, the supervention of great atmospheric heat may excite in them that excessive activity resulting in cholera. Hence it appears that what is a predisposing cause in one person may be an exciting cause in another, and *vice versâ*. The various causes of cholera are, I believe, essentially the same as those productive of diarrhœa; but, whereas one only of the causes hereafter mentioned may very often be productive of diarrhœa, the sources of cholera are more frequently complex, and thus effect, by their co-operation, results which, acting singly, they would be unable to produce.

It has been stated by Dr. Farr, the eminent medical statistician, that diarrhœa is as constantly observed in English towns when the temperature rises above 60 degrees Fahrenheit as are bronchitis and catarrh when the temperature falls below 32 degrees. In like manner, of all the influences which conduce to the development of cholera none is so potent as that of *Solar heat*. This truth is made manifest by the following facts:—

During the epidemic of cholera in England in 1831–2 the total number of deaths from the disease was 30,924; but of these no less than 24,613 occurred during the five months, June, July, August, September, and October. Again, during the epidemic of 1848–9 the total number of deaths from cholera in England was 54,398, and of these 50,521 occurred during the five months, June, July, August, September, and October. The epidemic which occurred during 1884 in the South of France

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and in Italy was restricted to the months just named. In Bengal the hot seasons are those of the worst cholera epidemics. Observations extending over a period of eight years prove that the most fatal cholera months for European troops are from April to September. Sometimes, however, cholera presents itself later in the year, as it did in the French capital in 1884.

But when the comparatively slight epidemic of cholera which prevailed in Paris in November, 1884, broke out the temperature was higher than it usually is in that month. At the observatory of Montsouri, which is at a much higher level, and therefore at a lower temperature, than that of the greater part of Paris, the maximum temperature in the shade from November 1 to November 7 rose each day as follows:—1st, 8·7 (Centigrade); 2nd, 10·7; 3rd, 13·4; 4th, 14·4; 5th, 16·1; 6th, 17·1; 7th, 19·2 (or 66·5 Fahrenheit). During the whole week ending November 7 the wind was southerly; and on the 7th it blew directly from the south. How exceptionally warm the weather was at that time was attested not only by the thermometer, but by the effects of the heat on vegetable life as far north even as Yorkshire. In the *Times* of November 8, 1884, there is the following surprising statement: "Strawberries are ripening in open gardens at Cranswick and Norton in East Yorkshire. Primroses are in bloom at Driffield, and, in the same neighbourhood, a pear-tree is bearing a second crop." Now it was during this period of exceptionally high temperature for the season of the year that cholera appeared. One of the first cases was reported to have occurred November 4; and during the nine days ending November 15 the disease had already destroyed 625 persons, or more than two-thirds of the whole of those who were victims of the epidemic. But, though the temperature began to fall on the 8th, the number of deaths from cholera during the week following November 15 was still considerable—viz., 238. It must, however, be borne in mind that many cholera patients who ultimately die linger a long time even in the algide stage, and that many more who pass through that stage into that of reaction assume a typhoid condition, which is often protracted during a considerable period, and which in many cases has a fatal termination. I have seen no record, and I doubt if any exists, showing what proportion of the total number of deaths from cholera during the epidemic in question occurred in the stage of collapse, and what proportion occurred during the stage of reaction; but at the London Hospital in 1866 a record of this kind was kept. During that year, 97 persons died of cholera in that hospital: of these, 58, or 59·8 per cent., died in the stage of collapse, and 39, or 40·2 per cent., died in the stage of reaction. Now it is probable that a like proportion of the total

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number of victims of the cholera epidemic in Paris in 1884 died in each of the two stages just mentioned. The total number of deaths was 986: 625, or 63·38 per cent., of these occurred during the first nine days after the disease declared itself; and 361, or 36·62 per cent., subsequently. Considering these facts in the light of the experience recorded at the London Hospital, I am led to believe that a large proportion of those persons who died of cholera after November 15, began to suffer from the disease before that date. It is especially worthy of notice that of the total number of deaths from cholera from the beginning to the end of the epidemic—viz., 986—863 of them occurred during the brief period of sixteen days extending from November 6 to 22. From the 23rd to the 29th the number of deaths had fallen to 74; during the following week it was only 21; and during the four following weeks, ending January 3, the total number of deaths was 14. After that date no death from cholera was registered. As evidence of the long lingering character of cholera in many cases when the disease assumes a typhoid condition, I may refer to the patient, already mentioned, who was the first to enter l'Hôpital de la Charité in November, 1884, was the last, or the last but one, of all the cholera patients treated at that time who left it.

The facts just stated respecting the temperature of the atmosphere during the early part of November, and the fact that while the force of the epidemic was mainly spent by the 15th of the month it was quickly afterwards exhausted as the temperature fell, seem to indicate unmistakably that there existed a causal relation between the temperature and the development of the disease. Nevertheless, it must be borne in mind that the temperature of 19·2° Centigrade, or 66·5° Fahrenheit, which was attained November 7, though exceptionally high for November, is a temperature which is exceeded in Paris every summer, and that though during every summer a large number of children are destroyed by what is called "infantile diarrhœa" (which I affirm to be cholera), and a few cases of "sporadic cholera" affecting adults occur, many summers pass without any manifestation of what is ordinarily understood as epidemic cholera. It is evident, therefore, that, however potent solar heat may be as a factor productive of cholera, there must be other powerfully co-operative factors as originators of it when it is developed while the atmospheric temperature is below 20° Centigrade (68° Fahrenheit). I shall presently refer to one of those factors which there are good reasons for believing co-operated effectively in producing the outbreak of cholera which destroyed nearly 1,000 persons in Paris during November, 1884.

When cholera, in its fully developed form, prevails, and,

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generally, preceding its advent in any given place, diarrhœa (cholera in its initial stage) is much more frequent than usual in the same place. A great increase of diarrhœa preceded and accompanied the invasion of cholera in 1832, in 1848–9, and in 1865. From June 3, 1865, when the first case of cholera was reported in London, to November 25, when the disease disappeared, the number of deaths from diarrhœa was 3,137, the deaths from cholera being only 182. The temperature of that year was at its highest in England throughout the month of July, and during that month the deaths from both diarrhœa and cholera were much the most numerous—viz., 1,284 from diarrhœa, and 75 from cholera. Moreover, the annual mortality from diarrhœa, like that from cholera, is greater in hot than in temperate climates. In England for the seven years 1848–54, it was at the rate of 86 per million; but for European troops in Bengal during the fifteen years 1830–45, it was at the rate of 4,555 per million. In fact, diarrhœa is destructive of life, especially during periods when, and in regions where, cholera is rife, to an extent which will surprise every one who is not familiar with the facts of the matter. Within the period of twenty years 1847–66 there were two cholera epidemics in England. During that period diarrhœa and cholera destroyed 417,199 persons, and of this number about three-fourths, or 311,200, were destroyed by diarrhœa. During the year 1857 the deaths in England from cholera were 1,150; but those from diarrhœa were 21,189. During the summer months of 1884 the deaths in Paris from cholera did not exceed one or two a week, but the weekly average number of deaths from diarrhœa, chiefly infantile, during each month, was as follows:—In May, 56; in June, 67; in July, 209; in August, 224; and in September, 110. From the evidence here tendered it appears, then, that great solar heat is the chief cause of both cholera and diarrhœa; that when cholera prevails diarrhœa is increasingly prevalent, and that where cholera is endemic diarrhœa is endemic also.

Now, seeing that solar heat of a certain intensity induces cholera, and seeing that, as a rule liable to certain exceptions, the number of deaths from the disease increases and decreases with the rise and fall of the atmospheric temperature, we must conclude either that heat alone suffices to produce in the nervous centres, *directly*, that condition of superabundance of blood associated with great excitement which is the immediate cause of the phenomena of cholera, or that it produces that condition, *indirectly*, in some inscrutable way—for example, by originating a poison in the blood. I venture to affirm that the first of these hypotheses claims our preference, because of its simplicity, because it accords with all known facts in respect to the action of

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solar heat on the nervous system, and because it suffices to explain, by the agency of a known factor, the origination of the condition in question without the necessity of conjuring up an additional agent, the presence of which afterwards becomes much more embarrassing than helpful. The following facts are, indeed, thoroughly established: Summer diarrhœa is induced by heat; cholera is induced by heat; in the great majority of cases of cholera, especially those occurring in temperate climates, the initial stage of the disease consists of diarrhœa exactly like summer diarrhœa; summer diarrhœa is often associated with cramps and notable coolness of the skin very suggestive of cholera; and, indeed, in the United States summer diarrhœa, the victims of which are chiefly children, assumes the likeness of cholera to such an extent as to have obtained the name *Cholera Infantum*.

The opinion has been expressed by C. T. Kiërulf, near Bergen, by Mr. Orton, in 1832, by Professor Parkes, who relates a case in support of his belief, and by Professor Aitken, who adopts it, that the diarrhœa which generally prevails during invasions of cholera is capable of infecting healthy persons with "true cholera;" but I doubt if any one except these gentlemen would affirm that the ordinary summer diarrhœa in Europe, the *cholera infantum* of the United States, and the endemic diarrhœa of India are the consequences of a blood-poison. And yet, wherever cholera prevails, either summer diarrhœa develops into cholera or the symptoms of the initial stage of most cases of cholera are to all intents and purposes identical with those of summer diarrhœa. The question therefore arises: at what stage does the sufferer from summer diarrhœa, or from the diarrhœa which is the prelude of cholera, become the victim of the alleged cholera poison? I believe all physicians having practical acquaintance with cholera are of opinion that the progress of the disease may be arrested in a large proportion of cases if prompt treatment be resorted to during the initial stage of diarrhœa; but would this be possible if the disease is from its beginning the result of a poison? And if it is not, I again ask, at what stage of the disease, and how, does the poison enter or become developed in the patient? I would also ask, how it comes to pass, if cholera be the result of a blood poison, that many patients recover from the disease in a wonderfully short time. And how can we explain the well-known fact that mental emotion can exert so powerful an influence on the sufferer from cholera as rapidly to hasten either his death or his recovery? Fear is as little likely to render the alleged poison in the blood more intensely poisonous, as is assurance of recovery to neutralize that poison. Mental emotion exerts no such influence on the progress and termination

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of those diseases which there are valid reasons for believing to be results of blood-poisoning—viz., typhoid fever, scarlet fever, or small-pox.

Wide Ranges of Temperature, along with a high temperature, are also peculiarly conducive to the development of cholera. In the thirty-sixth week of 1854, when cholera raged in London, and the deaths from all causes rose to their maximum (3,413), the maximum range of temperature was 38·1, and the average daily range was 30·9, *the greatest in the fifty-two weeks of that year.* Extensive observations in India prove most conclusively that, as a rule, when the range of temperature is greatest the deaths from cholera are much the most numerous. Now, is there anything in this remarkable and well-attested fact favouring the idea that cholera is the product of either microbes or a blood-poison? Is it conceivable that wide ranges of temperature can conduce to originate either one or the other? On the other hand, it is easy to understand how the dynamic influence of the cause in question produces an instability of circulation in, and therefore preternatural excitability of, the nervous centres—conditions alike conducive to the production of cholera.

Disturbances of Atmospheric Electricity are, it is believed, especially favourable conditions for the development of cholera. Many delicate, nervous women are painfully affected by such disturbances, which, according to the testimony of several authoritative medical observers, exert a powerful influence in producing puerperal convulsions, and in causing that disease to assume an epidemic character. In proof of the great influence of these disturbances on the nervous system I may mention that, in Paris, after the last Revolution, when the hospitals were crowded with the wounded, a very severe thunderstorm came on, and that the mortality was greater in all the hospitals on the night of the storm than on any previous or subsequent occasion. I have become acquainted with several cases in which persons in the presence of thunderstorms become troubled with diarrhœa; and of all the symptoms produced by attacks of atmospheric electricity diarrhœa is the most frequent. Moreover, it has been thoroughly ascertained that nearly every symptom of cholera is observable in one or another of the patients struck by lightning. It is recorded that at St. Petersburg during a cholera epidemic the magnetic needle did not obey its usual natural attractions, and that a magnet which usually sustained a weight equal to about seventy-five pounds to the square inch gradually lost this power as the disease increased, until at last, when the disease was at its height, it had only the sustaining power of fifteen pounds. As the disease decreased the power

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of the magnet increased until it sustained its proper weight. A similar fact was observed in Ireland during the epidemic of 1849. That cholera often succeeds a severe thunderstorm is well known. It seems impossible to avoid recognizing the existence of a causal relation between facts of this kind and the origination of cholera; but while such facts enforce the conviction that atmospheric electricity, which has, at least, close affinities with nerve force, exerts, when in a state of perturbation, a powerful and pre-eminently exciting influence on the whole nervous system, they are far from either suggesting or countenancing the notion that that influence poisons the blood or generates microbes in the intestines.

It seems to me likely that when the mode of attack of cholera is gradual, when it is characterized by premonitory diarrhœa, and when it presents itself over a wide area, the main factor in its production is solar heat, which, as already shown, acts the most potently when the diurnal range of its intensity is especially great; and that when the mode of attack is sudden, concentrated, and intense, the main factor in its production is that form of solar force we call electricity. Reasons have been adduced for alleging that there is probably a causal relation between the periodical famines which have been wont to occur in India, and a special solar influence in those years in which sun-spots appear. At the meeting of the British Association at Montreal in 1884, in the course of a discussion on the connection of sun-spots with terrestrial phenomena, Mr. E. D. Archibald stated that "in years of sun-spots the barometric pressure increases, *giving clear skies and great heat;*" and the Rev. S. J. Perry said that "there is a decided connection between solar spots and terrestrial magnetism, that he can foretell aurora, and that *the greatest magnetic disturbances occur when the largest spots are seen.*" Countenanced by these statements, the idea will not, perhaps, be considered far fetched, or, indeed, improbable, that those years in which cholera is *at once extensively* and *intensely* epidemic will be found to be sun-spot years. Certainly the subject is fraught with intense interest, and needs to be thoroughly investigated.

An especially remarkable and very interesting proof of the existence of a cosmical influence, rendering the atmosphere of those localities where cholera is epidemic unhealthy, is afforded by the fact that birds leave such places. The occasions have been so numerous and in so many different countries, that it is impossible to regard the coincidence as merely accidental. In 1846, on the occasion of the appallingly sudden outbreak of cholera at Kurrâchee, the birds (chiefly crows and vultures, which, hovering about the town, act as scavengers) left the

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place. Even the common house-flies (one of the plagues of Scinde) disappeared from the houses and tents at the same time. As the epidemic declined the birds and flies gradually reappeared.* In 1848, birds of all kinds deserted the towns of St. Petersburg and Riga, and it was in that year that cholera broke out there. The same phenomenon was observed, and chronicled at the time, in Western Prussia in 1849, and in Hanover in 1850. In the little town of Przemysl, in Galicia, all the jackdaws took flight from the streets into the country on September 26, 1872, and cholera broke out there two days afterwards. On November 30 these birds returned from their spontaneous quarantine, and by that time the last case of cholera had been recorded. In 1873 the disease appeared in Munich and Nuremberg, and not only the larger birds, but the sparrows and swallows, deserted both these towns. The inhabitants of Nuremberg looked with joy on the return of the sparrows, which occurred as soon as the cholera had disappeared from the town.† P. Hinckes Bird, Medical Officer of Health at Lytham, states that a physician, referring to the presence, about thirty years ago, of epidemic cholera in "a favourite sea-side resort," observed that the atmosphere appeared to be poisoned, for both the fishmonger and butcher told him that the fish and meat would not keep beyond a few hours; and, remarked one of them, casually, "Have you noticed that all the birds have left?" The physician added, "True enough, not one was to be seen or heard in the place, and the first sign of an improvement in the sad state of things was a return of the feathered songsters." A similar fact was observed at Scutari during an outbreak of cholera there.‡ "On the afternoon of July 23, 1883, at about 5.20, a marked change in the atmosphere took place" at Cairo, and on that day "birds seemed to have deserted the city, or, when still present, all signs of activity among them were suspended. On the night of the 23rd, cholera appeared amongst our men at Kasr-en-Nil, the citadel, and at Abbasieh."§ In the letter of the Marseilles correspondent of the *Times*, published August 2, 1884, occurs the following sentence:—"It is said that swallows desert a district afflicted with cholera, and it is certain that there are none just now in Marseilles." At a subse-

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* These interesting facts were communicated to me by an eye-witness of them—Lieut.-Colonel F. T. Ross—to whom I am also indebted for several other not less important facts which he observed during his residence in India, and which will be mentioned in subsequent pages of this essay.

† The *Times* (quoted from the *Globe*), September 3, 1874.

‡ *Ibid.* September 8, 1874.

§ Brigade-Surgeon McDowell's Report to Surgeon-General Hunter.

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quent page I shall suggest an explanation of these remarkable migrations.

Mr. Glaisher, in his report on the Meteorology of London during the three cholera epidemics of 1832, of 1848–9, and of 1853–4, says:—"The three epidemics were attended with a particular state of atmosphere, characterized by a prevalent mist, thin in high places, dense in low. During the height of the epidemic, in all cases the reading of the barometer was remarkably high, and the atmosphere thick." Lieut.-Colonel Ross states that during the burial of the first victim of cholera at Kurrâchee in 1846 (a soldier who was attacked early in the morning of June 14, who died within three hours, and who was buried in the afternoon of the same day), "a sort of white mist was seen to rise out of the sea and to pass over the burial-ground while the burial-service was going on, and that two soldiers attending the service fell down suddenly—attacked with the worst form of cholera. From that moment," he says, "the disease declared itself with extraordinary violence, and destroyed upwards of forty men the following night." The colonel was strongly impressed by the remarkable character of this mist, which, indeed, he spoke of as a "shroud." On the day preceding the night when cholera broke out at Cairo in 1883, there was noticed in the atmosphere a haze "of a yellowish colour, *suggesting the Arabic name for cholera, the translation of which means 'yellow air.'*" It seems as if some men know, as well as birds do, when cholera threatens; for at Ramleh (Egypt), August 3, 1883, when the temperature was high, and the moisture in the air approached saturation point, "*Men who had served in India remarked that there was cholera in the air, and at* 10.30 A.M. *the first case was reported.*"

The Absence of Ozone in places where cholera prevails has often been observed. Schönbein, whose elaborate researches concerning the nature of ozone, which he discovered in 1840, are well known, noticed, at Berlin, that the absence from the atmosphere of this remarkable form of oxygen coincided with the increase of cholera, and that the presence of it coincided with the decrease of the disease in that city. In 1855, at Strasbourg, the invasion of cholera coincided with a period of antozone, and the decline of the epidemic was accompanied by a return of the ozone; and in 1865 the same negative phenomenon was noticeable in England: in districts in which cholera prevailed not the smallest trace of ozone was discoverable by the test papers. At Marseilles, during the month of July, 1884, when the cholera epidemic was at its height, the average quantity of ozone in the air was 0·86 of a milligramme in 100 cubic metres of air; whereas, during the corresponding month of 1883 the

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quantity was 2·17. During November, 1884, when cholera prevailed in Paris, ozone was almost wholly absent, and during two days was wholly absent, from the air.

At the Observatory of Mont Souris, Paris, the average quantity of ozone each day during November, 1883, in 100 cubic metres of air, stated in milligrammes, was 1·8. But during the period from the 16th of October to the 13th of December, 1884, it was as follows:—

	Milligramme.
From 16 to 22 Oct.	0·9
„ 23 „ 29 „	0·7
„ 30 Oct. to 5 Nov.	0·4
„ 6 to 12 Nov.	0·3
„ 13 to 14 Nov.	0·0
Nov. 15	0·4
„ 16 to 22 Nov.	0·6
„ 23 „ 29 „	0·8
„ 30 Nov. to 6 Dec.	1·2
„ 7 to 13 Dec.	1·5

It is not unlikely that the absence from the air of the amount of ozone which it contains when in a normal state may be one of the conditions *pre-eminently* conducive to the generation of cholera. But this subject is still very obscure, and needs to be investigated much more thoroughly than it has been already before any satisfactory conclusions concerning it are likely to be reached. It is, however, well known that in proportion to the amount of ozone in the air, the process of oxidation is intensified, and that the more complete the oxidation of the blood, the greater is its facility of movement through the capillary blood-vessels. It is, therefore, not unlikely that a deficiency of ozone in the atmosphere favours that congestive hyperæmia of the automatic nervous centres which is the condition precedent of cholera; but a recognition of the existence of a causal relation between such a deficiency of ozone in the air and the development of cholera does not help us in the least to conceive how the alleged cholera-poison is produced.

In 1884 I received a letter from a friend mentioning that in India, medical men often send up pieces of raw meat to a considerable height in the air by means of a kite, and that in places where cholera is epidemic the meat when brought down shortly afterwards is in a state of incipient putrefaction. I have seen no mention of facts of this kind in books; but that they are observable in the region of cholera epidemics, at least in India, seems indubitable. Lieut.-Colonel Ross has informed me that at Kurrâchee during the epidemic of 1846, "the meat became tainted in a few hours," and that

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"meat attached to a kite which was sent up two or three hundred yards in the air came down putrid within ten or fifteen minutes of the time when it was sent up"! As it is now known that ozone is not only a powerful disinfectant, but is also a powerful preventive of putrefaction, the facts just mentioned are at once rendered credible and explicable by the justifiable assumption that over regions in which cholera prevails, not only the strata of air close to the earth's surface, but those considerably higher up, are at the time devoid of ozone. When such strata are at the high temperatures common in India, meat suspended in them is likely to putrefy with astonishing rapidity.

It seems to me that even the present state of our knowledge, imperfect as it is, concerning the causal relations of ozoneless air to cholera warrants the suggestion that during the summer and autumn months the amount of ozone in the air over every town ought to be ascertained every day, and that when that amount is found to be so reduced as to approach danger-point, special efforts should be made to prevent the generation and development of cholera. This remark is also, to some extent, applicable to atmospheric temperature when it rises to an unusual height.

While adverting to ozone, I am tempted to observe that an accurate report of the daily amount of ozone in the air of those countries (Australia, for example) in which epidemic cholera is not known to have appeared, might help to solve the very interesting problem—Why do those countries continue free from cholera?

Lowness of Site is a condition remarkably favourable for the invasions of cholera. Dr. Farr, who has fully investigated this subject, observes: "The elevation of the soil in London has a more constant relation with the mortality from cholera in London than any other known element. The mortality from cholera is in the inverse ratio of the elevation. The mortality of the nineteen highest districts was at the rate of 33 in 10,000 and of the nineteen lowest districts 100 in 10,000." The deaths were at the rate per 10,000 as follows: In districts at an elevation of 20 feet, 102; at from 20 to 40 feet, 65; at from 40 to 60 feet, 34; at from 60 to 80 feet, 27; at from 80 to 100 feet, 22; at from 100 to 120 feet, 17; at from 340 to 360 feet, 7. "The most favoured seats of cholera all over the world," says Dr. Goodeve, "are places not high above the sea; along the banks of rivers, and the estuaries of great streams." This established fact—that in proportion to the lowness of site of any locality it, *cæteris paribus*, conduces to the presence of cholera—offers, in my opinion, a far more intelligible and satisfactory explanation of its frequency and of the extent of its ravages along the borders of

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rivers and the main lines of human traffic than does the common allegation that cholera "travels" or is propagated along these lines by contagious influence. Experience has long ago established the fact that in proportion as the air we breathe is pure it conduces to health, and that as a rule low sites are less healthy than those which are lofty. Moreover, students of cholera know that in proportion as the general health of individuals is impaired are they likely during the prevalence of cholera to become its victims. But in whatever manner and to whatever degree lowness of site may operate as a predisposing cause of cholera, this condition certainly cannot be regarded as a toxic cause, unless the lower strata of the atmosphere in low-lying regions of the earth's surface are also, as a rule, to be regarded as poisonous.

Prolonged Marches of soldiers in India facilitate in a remarkable degree the development of cholera: it was proved by Dr. Balfour that of the native soldiers of the Madras army thirty-two died of cholera in cantonment and eighty-six when marching, to an average of 10,000 in strength. Moreover, it has been ascertained that the longer the marches the more frequent the attacks. Many facts of this kind are recorded. I have been informed by a French physician that the marches of French soldiers in Algeria operate as an exciting cause of cholera to an extent appallingly impressive. But it is not only soldiers who are attacked on march: it is well known in India that people of all conditions who travel on foot are specially liable to be attacked, and that pilgrims, on their way through Lower Bengal and Orissa, strew the road to Juggernaut with their bones. Walking in hot weather, most especially when the back is exposed to the sun, induces in the spinal cord a condition of hyperæmia which is extended to the collateral ganglia of the Sympathetic, and thus a state of the nervous centres conducive to the onset of cholera is established.

Bad Food and Eating to Excess have often converted a tendency to cholera into its reality. Half-putrid fish, bad shell-fish, bad pickled pork, are known in several cases to have been followed by violent attacks of cholera. Dr. Carpenter mentions "an outbreak of cholera and choleraic diarrhœa among a number of school-children who had eaten plentifully of spoiled oysters, and by which eleven of the sufferers lost their lives." It is recorded that immediately after the arrival of a cargo of bad oysters in New York diarrhœa and cholera prevailed to a great extent. Unripe fruit and crude vegetables, and even wholesome food eaten to excess, will operate as exciting causes of cholera when atmospheric conditions predispose to it. It has been observed that cholera seizures appear to be especially frequent in natives of India after a full meal. It is well known that her Majesty's

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14th Regiment when at Berhampore was attacked with cholera after it had received an allotment of prize money. The disastrous effects of irritants of this kind have often been observed, but no one dreams that, while thus acting, they are capable of generating an infective poison in the blood.

Alcoholic Drinks are powerful aids in producing cholera. It was found that during the cholera epidemic of 1848–9 in England the deaths from cholera on Saturday, Monday, Tuesday, and Wednesday were above, and on Thursday, Friday, and Sunday, below the average. The weekly wages are generally paid on the Saturdays, and the Mondays in London and other cities are days on which a certain proportion of the population indulge in intoxicating drinks. During the epidemic of 1865 the mortality in Berlin suddenly rose on certain days, and was clearly referable to excess in drinking. Dr. Letheby has pointed out that the daily record by the Registrar-General of the deaths in London, from cholera and diarrhœa respectively, during the seven weeks ending September 22, 1866, showed decisively the fatal effects of alcohol. According to that record, "for every 100 deaths from cholera that occurred on the daily average of Sunday and Monday, 122 occurred on Tuesday, 102 on Wednesday, 105 on Thursday, 88 on Friday, and 93 on Saturday; and so also of diarrhœa, the proportional numbers being 100, 134, 125, 126, 109, and 111: as if the dissipation of Saturday rendered individuals more susceptible of the disease which attacked them on Sunday or Monday, and killed them on Tuesday, after which the force of the malady again declined until the following Tuesday."* When the epidemic of cholera broke out at Naples in the beginning of September, 1884, the *Pungolo*, a Neapolitan newspaper, attributed the increased number of cases to intemperate living on the last day of the preceding month, which was a *fête* day.

Sudden manifestations of an increase of cholera cases as effects of even one day's excessive drinking are established facts, so that alcohol, whether taken habitually or only on special occasions, is proved to be capable of acting either as an exciting or predisposing cause of cholera. The blood-poison, zymotic, and microbe theories of the disease afford no explanation of this fact. But just as in respect to opium, the pathological doctrine I have put forward at once explains and is confirmed by the fact in question, the special affinity of alcohol for the nervous centres, its great exciting influence on those centres, and its power of producing vomiting and purging in many cases, when taken in considerable quantities, are well-ascertained facts; it therefore not only produces, in an especial degree, hyperæmia of the nervous centres,

* "London Hospital Reports," p. 490.

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and thus, according to my doctrine, an especial predisposition to cholera, but often also two of its leading symptoms.

Opium is now generally admitted to exert great power in conducing to the development of choleraic collapse of a type peculiarly fatal. This is the conclusion of numerous and eminent authorities—men who have had a large experience of the treatment of cholera in India. Opium is often prescribed, and not infrequently with seeming advantage, during the stage of premonitory diarrhœa; but in a large proportion of cases, even in this stage, its failure to arrest the progress of the disease is signal. Referring to my assertion and explanation, in 1865,* of the dangerousness of opium, Professor Maclean, of Netley Hospital, said "he could bear the strongest testimony, not only to the inefficacy, but to the very great danger in the use of opium in cholera." Professor Goodeve and Dr. Macpherson have expressed themselves to the same effect; and the medical journals contain many reports of the power of opium to favour or induce the development of cholera after the disease has fairly set in. Now, opium is known to have a peculiar affinity for, and to exert a specific influence over, nervous tissue. Therefore, when co-operating with other causes, or when acting alone in the production of choleraic collapse, it does so, as will generally be admitted, by virtue of its great and peculiar power of modifying the vascular and functional condition of both the cerebro-spinal and sympathetic nervous centres. The pathology of cholera already expounded reveals for the first time the *modus operandi* of opium in developing the disease; while the admitted facts that opium has often induced collapse, that collapse so induced is more than ordinarily fatal, and that when reaction succeeds to it, that reaction is often unusually protracted and dangerous, constitute additional evidence of the truth of the pathology in question.

Purgative Medicines have acted as the exciting causes of many attacks of cholera. This assertion, considered in connection with what has just been said of the power of opium to do the same, will no doubt seem paradoxical; but that it is nevertheless true is proved beyond the possibility of doubt. Ample evidence to this effect is presented in my volume on "Diarrhœa and Cholera" (p. 164 *et seq.*); but I must content myself with mentioning several physicians, mostly Anglo-Indian, whose experience constrains them to testify to the truth of this statement—namely, Dr. Twining, Dr. Morehead, Dr. Macintosh, Dr. Painter, Dr. Durham, Sir Ranald Martin, Dr. Macpherson, Dr. Barlow, and Professor Goodeve. In fact, that it is dangerous to take aperients

* In the course of an address to the members of the Hampshire Medical Society, at Southampton.

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when cholera is prevalent has become, as Dr. Macpherson says, "the common doctrine of Europe, and the latest experience in France confirms it." The Medical Council of the London College of Physicians found that the treatment of cases in collapse by castor oil resulted in a mortality of 77·6 per cent. And yet Dr. George Johnson's great remedy for cholera is castor oil! As his high professional position lends importance to his advice, and therefore, in this case, makes it the more dangerous, it is necessary that this danger should be distinctly pointed out. "With regard to castor oil," writes Dr. Macnamara, "I was acting as house-physician to King's College Hospital, in 1854, when Dr. G. Johnson was treating his cholera patients on eliminative principles. I caught some of his enthusiasm on the subject, and came out to India the same year full of confidence and hope in castor oil. . . . . In the following year I was left at Bhangulpore in charge of a field hospital. I was the only medical man in the place, when cholera burst out among the Europeans and natives under my care. I went boldly to work with castor oil, but it absolutely and completely failed; *the mortality from the disease was fearful.* I have since, on several occasions, tried castor oil in cholera, but I have now finally abandoned it, having never seen any benefit arise from its use." The *modus operandi* of purgative medicines is, I presume, now generally recognized as being through the agency of the nervous system, even in those cases in which the medicine enters the circulation. The enteric nervous centres are, in any case, unduly excited, become foci of an excessive afflux of blood, and thus preternaturally energetic. Now this is precisely one of the conditions which obtains during an attack of cholera, and which, when the causes already mentioned are exerting their influence, is likely to induce the disease. And, conversely, when, by the action of purgatives, the functions of the enteric nervous centres have become intensified, the hyperæmic condition of those centres only needs the co-operative influence of either excessive heat or some one of the other epidemic agencies to ensure the development of the disease. A consideration of the *modus operandi* of purgative medicines here indicated, in connection with the pathology of cholera already expounded, explains at once why, in cholera times, their use is so beset with danger, and how, not unfrequently, they become potent causes of the disease itself.

Painful Dentition.—The process of teething, which, by the excessive irritation of the dental nerves, and the consequent hyperæmia of the medulla-oblongata (the topmost part—which is within the skull—of what Marshall Hall called the "true spinal cord") in which they converge, is, in ordinary times, a most fruitful source of diarrhœa, and becomes in cholera times,

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and often in those summers when adults have a complete immunity from cholera, a very active cause of the disease in children. My readers are now aware that many factors are concerned in the production of cholera. Sometimes several co-operate, sometimes not more than two, and sometimes the disease is engendered, there are reasons for believing, by one alone. Many of these factors can act as predisposing or exciting causes of cholera only when two or more act together. Many children pass through the ordeal of teething with but little pain, and without suffering grave consequences of any kind during any part of the year; and in temperate climates those children in whom dentition is decidedly painful, and the cause of more or less constitutional disturbance, generally escape any serious consequences from that process during the winter months. But during the summer months such children are in great danger. Their rapidly growing nervous systems, like those of children generally, are already suffused with blood to a maximum degree in order to supply material for their exuberant nutrition, and are therefore in a condition especially predisposing them to be easily wrought up to a pitch of morbid excitability; the painful dentition of which they are victims supervenes as an exciting cause of additional hyperæmia of the nervous tissue, and the high atmospheric temperature of the summer months co-operating with these causes suffices to induce that morbidly exalted state of circulation in, and tumultuous excitement of, the nervous centres which induces that fearfully fatal disease called in Europe infantile diarrhœa and in America *cholera infantum.*

Noxious Effluvia are powerfully co-operative with solar heat in the production of the disease. As Dr. Goodeve justly says: "In spite of exceptions, the places in which the air is most vitiated from privies, cesspools, drains, decaying animal and vegetable refuse, or from over-crowding and concentration of human emanations, are those in which cholera has generally been most fatal and most widely spread." The terminal branches of the sensory nerves spread over the large surface of the pulmonary mucous membrane, transmit directly to the nervous centres the unhealthy impressions made on them by foul emanations or noxious effluvia; and as the primary receptive surface of these impressions is especially great, the converging effects produced on those centres are great also, and hence the profoundly depressing influence through the myriad paths of reflex action exerted on the whole organism.

Impure Water is, probably, of all avoidable causes of cholera, the most common and the most baneful. In proof of its potency I shall mention only one experiment, which was performed several years ago on a large scale, and which was as free as

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possible from error. The population experimented on numbered between 400,000 and 500,000. The Lambeth Water Company drew its supply from the Thames at Ditton, above the influence of the London sewage and tidal flux; the Southwark and Vauxhall Company drew its supply from the river near Vauxhall and Chelsea. The water of the Lambeth Company was tolerably pure; that of the Southwark and Vauxhall Company was very impure. The water of both companies was distributed in the same district at the same time and among the same class of people, the pipes of the two companies being laid pretty evenly in the same areas, in many places running side by side in the same streets, and the houses supplied being pretty equally distributed. The deaths in the houses supplied by the Lambeth Company were at the rate of thirty-seven, and in the houses supplied by the Southwark and Vauxhall Company at the rate of 130 to every 10,000 persons living. It thus appears that of the drinkers of the foul water about three and a-half times as many as those who drank the purer water died of cholera.

The explanation given above of the mode of action of noxious effluvia is precisely applicable to the mode of action of bad water in causing cholera, the difference being only that whereas noxious gases act on the terminal branches of the sensory nerves of the pulmonary mucous membrane, bad water acts chiefly on the terminal branches of the sensory nerves of the alimentary mucous membrane. I am well aware that noxious gases and bad water may be said with much reason to exert their pernicious influence by poisoning the blood. They are supposed to do so when they engender typhoid fever; but even typhoid fever is not contagious, and with respect to their action as causes of cholera it must be borne in mind that they can, and, therefore, probably do, operate chiefly in the manner explained above, and therefore as excitants of the nervous system in the same manner, and with the same result, as are characteristic of all the other causes of cholera already passed in review; and this conclusion is justified by the consideration that inasmuch as those causes are proved to operate dynamically in either predisposing to, or in effecting the development of, cholera, so it is probable that noxious effluvia and impure water, when causative of cholera, operate in the same way.

Nocturnal influences favouring the advent of cholera have often attracted attention. It has been noted by many observers that cholera begins most frequently during the night, and especially between two and four o'clock in the morning. I have elsewhere*

* "Diarrhœa and Cholera," p. 172 *et seq.*

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enumerated several circumstances, among others the fall of temperature during the early morning hours, which conduce to this result; but here I can only advert to that cause which I regard as the most potent. In my work on neuralgia* I have adduced proofs that during normal and profound sleep there is a maximum afflux of blood in the automatic nervous centres, the brain being meanwhile anæmic. Now, during the hours immediately after midnight, sleep is, as a rule, the most profound; and, meanwhile, the spinal cord and sympathetic nervous centres are in a state of hyperæmia. This condition, when excessive, is, as I have shown, the proximate cause of cholera; and, therefore, while it obtains in a lesser degree during sleep, those nervous centres are at that time susceptible, in a maximum degree, of those influences which originate cholera. Hence it is that this disease, as well as epilepsy, is most prone to surprise its victims in the night.

Fear, that especially potent cause of cholera, needs only to be named in order to suggest how exclusively it does its deadly work through the agency of the nervous system. There is an old and well-known story of an encounter outside an Eastern city between the plague-demon, when about to enter the city, and a citizen. The citizen asked the demon what he was going to do there, and the demon said he was going to slay three thousand people. When the demon came out of the city, the citizen taxed him with lying, for thirty thousand people had been slain. The demon replied, "But I only slew three thousand; fear slew the rest." This story contains a great truth, which is attested by many medical witnesses in India, Europe, and America. Innumerable observations prove that those persons who are depressed or alarmed are most likely to become victims of cholera, that in the presence of a cholera epidemic panic intensifies its force, and in many instances speedily develops simple diarrhœa into cholera itself, and that confidence proves helpful both in warding off an attack and in the struggle for life of patients already in actual danger. When at Southampton during the epidemic of 1865 I was fully assured by observations in several cases of the great influence which fear, on the one hand, and confidence, on the other, exerts on the progress and termination of the disease. Sir Thomas Watson, referring to diarrhœa, says, "A curious exciting cause is to be found in *mental emotions*, and especially the depressing passions, grief and, above all, fear. A sudden panic will operate on the bowels of some persons as surely as a black dose, and much more *speedily*." It is well known that many

* "Neuralgia and Kindred Diseases of the Nervous System: their Nature, Causes and Treatment." London: 1873, p. 171.

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soldiers, especially young ones, are attacked with diarrhœa when going into battle. Several cases have been observed in which diarrhœa has occurred almost immediately after a railway collision.* In short, as pithily remarked by Dr. Forbes Winslow, "during an attack of cholera the patient who has the least fear of dying has, *cæteris paribus*, the best chance of living." Can these facts be accounted for by the organic poison or microbe hypothesis? It is impossible to answer this question except in the negative.

In cases of the kind here referred to, congestion of the nervous centres appears, physically speaking, to be the primary fact; but I apprehend that the order of causation is as follows:—Terrifying or exciting impressions, suddenly communicated, are conveyed to the sensory ganglia, and are thence distributed to the cellular structure constituting the cerebral convolutions; these are thrown into tumultuous excitement, which is propagated along the motor tracts with the rapidity of lightning down the whole spinal axis, and laterally to the ganglia of the Sympathetic; these becoming suddenly swollen with blood, instantly act with intensely vehement energy, and diffuse their subtle stimulus in all directions. As the source of the powerful impressions which they have, in this case, received is cerebral, so the *chief* direction in which the vaso-motor impulses are reflected is towards the brain; hence, quick as thought, the cerebral arteries are contracted with preternatural energy, and thus, in extreme cases, the brain being rendered comparatively bloodless, the person is stunned as if by a blow, the face becomes pallid, cold sweat sometimes exuding from it, if consciousness is not abolished, mental power is still greatly enfeebled, the temperature of the surface of the body is lowered, the stomach and intestines are preternaturally stimulated, their mucous membrane exudes its appropriate secretion superabundantly, their peristaltic action becomes excessive, and thus diarrhœa and cholera, originating in mental emotion, are clearly due to the same *proximate cause* as that which, as I have endeavoured to show, is operative in all the other cases in which the predisposing or exciting causes of the disease are widely different.

Insanity has been found to be conducive to the development of cholera. A physician acquainted with the facts has informed me that when a certain lunatic asylum was within the sphere of a cholera epidemic, a much larger proportion of its inmates than of the population outside were struck down by the disease. At the asylum for the insane at Marseilles, the disease

* "Injuries of the Spine and Spinal Cord, without apparent Mechanical Lesion." By H. W. Page. London: 1883, p. 162.

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was especially destructive in 1884; whereas the mortality from cholera in that city, as a whole, was 5 per 1,000, that in the asylum was 5 per 100.* The peculiar liability of the insane to be attacked by cholera is in striking accord with the theory above expounded; for, evidently, the nervous systems of persons suffering from cerebral disorder are sure to be much more excitable, and much more prone, therefore, to become excited and hyperæmic than are the nervous systems of persons in ordinary health.

Sex.—Of the 97 persons who, in 1866, died of cholera at the London Hospital, 52, or 53·6 per cent., were males, 45, or 46·4 per cent., were females; of 972 persons who were destroyed during the cholera epidemic of November, 1884, in Paris, 578, or 59·5 per cent. (three-fifths), were males, and 394, or 40·5 per cent. (two-fifths), were females; and of 339 persons who died of the same disease at the Pharo Hospital, Marseilles, in 1884, 34 per cent. were females and 66 per cent. (or two-thirds of the whole!) were males.† Until much more statistical information on this subject than at present exists is available, any generalization concerning it would be premature; but I may remark that there are at least two reasons why a greater mortality of males than of females from cholera may be expected. (1.) Persons who are in the habit of taking alcoholic liquors to excess are peculiarly liable to be attacked by cholera, and are, of all persons attacked, precisely those who recover with the greatest difficulty, and who therefore furnish the largest proportion of fatal cases. Now, men are much more prone than women to indulge to excess in the habit of drinking, and this fact is therefore one valid reason why it is likely that cholera is more destructive of males than of females. (2.) There is a much stronger tendency to suffer from diseases of the nervous system having a fatal termination in males than in females; and therefore, inasmuch as, according to the hypothesis which I have propounded, cholera is a disease of the nervous system, the greater tendency of males than females to become victims to diseases of that system explains why it is (apart from the fact that men drink more alcohol than women do) that more males than females are destroyed by cholera. I have extracted from the *Bulletin hébdomadaire de Statistique municipale* of Paris the records of death at all ages from diseases of the cerebro-spinal nervous system during the last fourteen weeks of 1884. The total number during that period is as follows:—

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* *Marseille Médical*, March 30, 1884, p. 176.

† "Rapport sur l'Epidémie de Choléra." Marseille: 1885, p. 128.

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				Males.		Females.
From birth	to	3 months	old ...	54	...	38
„ 3 months	„	1 year	„ ...	36	...	30
„ 1 year	„	2 years	„ ...	12	...	13
„ 2 years	„	5	„ „ ...	10	...	7
„ 5 „	„	15	„ „ ...	9	...	4
„ 15 „	„	35	„ „ ...	24	...	30
„ 35 „	„	60	„ „ ...	238	...	164
„ 60 „	and upwards		...	314	...	312
				697	...	598

Total number of deaths of both sexes ... 1,295.

It appears from this table that at every age mentioned in it, except two, the number of deaths of males exceeded that of females, and that of the total number of deaths of both sexes —viz., 1,295—the deaths of males exceeded those of females by 99. In other words, the deaths of males were 54 per cent. and the deaths of females 46 per cent. of the total mortality due to diseases of the nervous system. It may be alleged that a certain proportion of this excess of mortality of males may also be due to the fact that men drink more alcohol than women do, but, if we compare the mortality of boys with that of girls under fifteen years of age from diseases of the nervous system we still find that the former exceeds the latter, and even in a greater ratio than that which prevails in respect to persons of all ages: of the total number of persons who die under fifteen years of age from the diseases in question, it appears, according to the table just given, that 57 per cent. are boys and 43 per cent. are girls. It is therefore evident that there is a greater inherent tendency in males than in females to suffer and to die from diseases of the nervous system. In so far as this is so, the fact not only supplies an explanation why so many more males than females are destroyed by cholera, but it also constitutes an additional and peculiarly interesting argument in proof of the doctrine that cholera is essentially a disease of the nervous system. I may mention as bearing on the point now discussed, and as accordant with the view just expressed, that of the 1,684 children who, during the ten weeks from the middle of July to the end of September, 1884, died in Paris from infantile diarrhœa, 873 were boys and 811 girls. The excess of mortality of boys was therefore 62. Of the total number of deaths, 52 per cent. were of boys and 48 per cent. of girls.

In the present state of our knowledge it may be impossible to give the true answer to the question—Why do diseases of the nervous system prove more fatal to males than to females? I am tempted, however, to propose an answer which, provisionally,

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may perhaps seem worthy of acceptance as probably the true one—an answer derived from a consideration of the relative size of the male and female brain. The data for this consideration are supplied in the two following tables, for which I am indebted to the extremely valuable and important work of Professor Topinard:*—

TABLE SHOWING THE WEIGHT, IN GRAMMES, OF THE MALE AND OF THE FEMALE BRAIN AT DIFFERENT PERIODS OF LIFE.

	Males. Average weight.	Females. Average weight.
At birth	331	283
From 0 to 3 months ...	493	452
„ 3 „ 6 „ ...	603	560
„ 6 months to 1 year ...	777	728
„ 1 to 2 years ...	942	844
„ 2 „ 4 „ ...	1097	991
„ 4 „ 7 „ ...	1140	1136
„ 7 „ 14 „ ...	1302	1155
„ 14 „ 20 „ ...	1374	1244
„ 20 „ 30 „ ...	1357	1238
„ 30 „ 40 „ ...	1366	1218
„ 40 „ 50 „ ...	1352	1213
„ 50 „ 60 „ ...	1343	1221
„ 60 „ 70 „ ...	1315	1207
„ 70 „ 80 „ ...	1289	1167
„ 80 „ 90 „ ...	1284	1125

It is thus evident that the brain of man is considerably larger than that of woman. Professor Topinard gives several estimates; the difference varies in the different estimates from 126 to 164 grammes at the age from twenty to sixty, and from 123 to 158 grammes at the age from sixty to ninety. Moreover, the brain of man is not only heavier absolutely than that of woman; it is also heavier relatively to his stature than is that of woman relatively to her stature.

TABLE SHOWING THE RELATION OF THE WEIGHT, IN GRAMMES, OF THE BRAIN TO THE STATURE, OR THE RELATIVE INCREASE OF THE TWO.

Stature = 1000.

	Male.	Female.
At birth	0·716	0·672
„ 3 months	0·901	0·861
„ 3 to 6 months ...	1·046	0·980
„ 6 „ 12 „ ...	1·177	1·115
„ 1 year to 2 years ...	1·302	1·205
„ 2 to 4 years ...	1·365	1·235
„ 4 „ 7 „ ...	1·197	1·209
„ 7 „ 14 „ ...	1·091	1·010
„ 14 „ 20 „ ...	0·894	0·849
„ 20 „ 30 „ ...	0·800	0·784

* "Eléments d'Anthropologie gén.," par le Dr. Paul Topinard. Paris. 1885.

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Now, the nervous system generally is peculiarly delicate and impressionable; it is supplied with blood, or is vascular, to a pre-eminent degree; it is therefore peculiarly mobile, perturbable, and liable to become disordered by causes which exert little or no appreciable influence on the other parts of the organism. This being the case, it is evident that the greater the mass of this system is the greater is the sphere of operation of the numerous, various, and, in large measure, peculiarly subtle causes of disorder and disease. And inasmuch as the male brain is larger than is that of the female, its liability to be morbidly affected by those causes is in proportion to its comparatively larger size. There are, however, several subordinate influences which doubtless co-operate with the main influence just mentioned to produce the effect in question. For example, the much greater activity of the male than of the female brain as denoted by the much larger amount of mental work done by men than by women: men are, on the whole, subject to much greater mental strain than women in the struggle for existence; the vanity or ambition which animates each sex forces men to tax their mental powers more heavily and more persistently than women do; and, finally, men are exposed to a greater number and variety of dangers than women are during the ordinary vicissitudes of life. Moreover, whatever may be the amount and effect of these subordinate influences, the results of their co-operation become incarnated in the brain, and, by virtue of the law of heredity, are transmitted to succeeding generations, and thus increase and strengthen the tendency in question.

The rapid survey now accomplished of the proximate, predisposing, and exciting causes of cholera, presents, I venture to affirm, a complete and satisfactory explanation of the genesis of the disease, of every individual symptom of it, and of every *post-mortem* phenomenon observable in the bodies of those to whom it has proved fatal. This characteristic will be admitted by all scientific thinkers to be a strong argument in favour of its truth. When, moreover, it is also found to afford an explanation of the mode of action of every force and influence known to become, directly or indirectly, causative of cholera, and when it is borne in mind that no other theory yet put forward explains either the essential nature of the disease, or how the known causes of it operate, the presumption that the *rationale* here expounded is the true one amounts as nearly to certainty as is possible within a region in which mathematical proofs are not available. Now this theory implies in no sense whatever the existence of a cholera poison; and by accounting for every fact connected with the disease, not only without assum-

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ing the existence of such a poison, but in a manner which virtually negatives its existence, it presents evidence precisely as forcible as is that in proof of its own validity that cholera is originated and developed independently of any specific poison, microbe, or cholera germ, and that, therefore, *as causative of cholera*, they probably exist only in the imagination of certain pathologists and of those who too credulously accept their dicta.

SECTION V.

The Non-contagiousness of Cholera: Dr. Koch's Microbe possibly a Beneficent Agent; why it abounds in Cholera Patients.

If cholera is not produced by a specific poison, microbe, or cholera germ, it is not likely to be contagious, and that it is not so experience of it affords convincing proofs.

During the cholera epidemic in Paris in 1831 over 55,000 persons were attacked, and of these over 18,000 died. Of the fatal cases only 164 were cases of persons whose duties or profession called them to nurse, or prescribe for, the sick; and these 164 formed part of over 2,000 persons thus employed. At St. Petersburg, of 58 persons employed in the hospitals only one had the disease. At Moscow, out of 376 persons attached to the hospitals only six were attacked. During the epidemic of 1866 in New York, 123 out of 800 inmates of the workhouse died; but of the fourteen house physicians and surgeons who were employed in the institution, some of them being in constant attendance on the sick, not one suffered from the epidemic. At the London Hospital during the epidemic of 1866, the result was less favourable. "Out of somewhat under 130 persons engaged in attending the cholera patients and washing the sheeting, &c., from the cholera wards, seven were attacked by cholera; of these five died."* Dr. Goodeve, late Professor of Medicine in the Medical College, and first physician to the hospital connected with that college, at Calcutta, whose professional life has been mainly spent in India, states in his valuable paper on cholera that "the majority of medical men in India, accustomed to see cholera year after year, to be in constant intercourse with the cholera sick, and to see the general immunity of hospital attendants and of themselves, doubt the contagiousness." He adds, "I should, as far as my own experience goes, say that cholera does not

* "London Hospital Reports," vol. iii. 1866.

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spread from the sick to the whole by any rapidly acting emanation." Dr. Morehead's observations support the view of the non-spreading of cholera in hospitals through contagion. They were carefully conducted through three epidemics in Bombay, and, though he refrains from drawing positive conclusions, his facts are not in favour of contagion. Another important Indian authority, Sir Joseph Fayrer, states that "he had seen hundreds of cases of sporadic and epidemic cholera, but had seen nothing to make him believe there was anything of contagion in connection with the disease." Another and equally authoritative witness, Surgeon-Major T. R. Lewis, Assistant Professor of Pathology at the Army Medical School, Netley, whose experience of cholera was also gained in India, says "for fourteen years he had studied cholera, and had never seen anything to lead him to think it contagious. *It was the custom in India to treat cholera in the same wards as other diseases, and no evil resulted.*"

Surgeon-General J. M. Cunningham, Sanitary Commissioner with the Government of India, states that "from the record of about 8,000 attendants on cases of cholera in India it is proved that they suffer no more than other people living in the same place. There is no danger in attendance on cholera cases." This statement is cited by Sir William Guyer Hunter in his report on the cholera epidemic in Egypt, and he then adds:—

"My personal experience of cholera in India is in accordance with the opinions above expressed. The professional staff, a large body of students and attendants, of the Medical College and Hospital, Bombay, who were more or less in frequent communication with cases of cholera, and many of whom were also engaged from time to time in performing *post-mortem* examinations, appeared to enjoy comparative immunity from the disease without any special precautions being taken. Experience gained during the recent epidemic in Egypt confirms still further these facts. It was no uncommon thing to hear from medical officers and others that their clothing and persons had been covered with the discharges from cholera patients, which had been allowed to become dry; yet no evil results followed. Circumstances rendered it necessary that the British officers serving with the Egyptian army should attend on the cholera sick, wash the bodies after death, according to Moslem usage, and afterwards bury them, and yet in no single instance, if I am correctly informed, did they contract the disease."

Monsieur le docteur Poucel, in a paper read by him on August 2, 1884, to the National Society of Medicine at Marseilles, said:—

"We all know that of the staff of our two hospitals, including the medical officers, nurses, and attendants occupied in the wards containing the choleraic patients in 1865 and 1866 not one person was attacked by the disease. A like result was experienced at Cairo in

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1881, where more than 100 persons were occupied with the cholera patients; at Naples; at Mansurah, where of 80 hospital attendants not one was attacked; at Damietta, where of more than 60 attendants only one was attacked; and at Constantinople during 1855 and 1856, according to the report of Cazelas, the whole of the hospital staffs escaped the disease.

"And at present do we not see the same fact reproduce itself under our eyes at the Pharo [hospital]? Has a single case of cholera declared itself in the staff of the establishment?

"But I go further: not only has cholera not attacked the persons attached to the hospital service, but, during the month that this hospital has received cholera-patients, has it become a focus of the disease? If you consult statistics you will see that the quarter in which it is placed is the one of all the city which is, perhaps, the most spared, notwithstanding the little dirty houses, over-inhabited, the stagnant waters in the low part to the left, a population of children free from every hygienic rule: all the conditions, in short, are united for constituting a focus of cholera, and the focus is not constituted.

"I go still further: during the last month the Pharo is visited every day by numerous persons led there by duty, devotion, science, curiosity, or by political considerations; of this number, which it is difficult to estimate, but which is, certainly, very considerable, can we mention one single person who has contracted the disease there?

"This, it must be confessed, is a strange contagious disease which seems almost to spare the persons who come in contact with its victims! In presence of such facts, which may be multiplied indefinitely, we cannot read without surprise the assertions of the *Gazette des Hôpitaux* that 'cholera is one of the most contagious diseases in the world.'" Dr. Poucel adds that during 1884, "of more than 200 persons employed at Marseilles in connection with the hospital, only one, a carter, died of cholera, and he was not employed in emptying the depositories of fæcal matter."

Facts, however, of an opposite kind have been recorded; for example:—

"At Toulon, in 1865, 23 persons forming part of the staff of medical men and employés of the civil and military hospitals, died of the disease; in the same town during 1884, 19 were attacked, and of these 7 died. At the hospital for the insane at Marseilles 7 employés were attacked; two of these had been in contact with cholera patients. . . . .

"The first class of facts just given demonstrates that congregating cholera patients in a hospital does not necessarily create a focus from which the disease is spread.

"The second demonstrates that dwelling with cholera patients does not preserve from cholera the persons attending on them.

"It is necessary, therefore, to find out some other agency than that of contagion in order to explain the propagation of cholera."*

* *Marseille Médical*, Sept. 30, 1884, pp. 524–5–7.

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The recorded experiences of the epidemic of 1849 in England established the fact that the washers of large collections of linen soiled by cholera patients in many large hospitals did not suffer seriously in that year. It is especially worthy of note that the dejections of cholera-patients, the dangerousness of which is insisted on by nearly all writers on cholera, and particularly by Dr. Koch, do not seem to be really dangerous at all. It is well known that on the Continent the custom prevails almost everywhere of accumulating the fæces of each house either in a large vessel, or in a "fosse" (cess-pit), beneath the house. They are thence fetched away at intervals by men who are constantly employed for this purpose, and who are called *vidangeurs*. Now "statistical evidence has always proved that these men enjoy an almost complete immunity from cholera."* . . . . "At Marseilles in 1865, of 150 *vidangeurs* not one died; and in 1884, of 200 only one died."† An immense number of *post-mortem* examinations in cholera cases have been made in most places, the contents of the intestines being submitted to all kinds of examination, and, as is well known, without any evil consequences to the examiners. Professor Aitkin says: "During my experience as demonstrator of anatomy in the University of Glasgow, for a period of six years (including the severe epidemic of cholera there in 1848–49, and during which time almost all the subjects for dissection had died of cholera), not a single student suffered from cholera." The experience in Edinburgh was, according to Dr. Alison, precisely similar. "It is certain that the dissecting rooms there were supplied during the greater part of 1848-49, as they were in 1832, almost exclusively by cholera subjects, and in neither year was there a single case of the disease among the numerous students attending the rooms." In fact, the intentional inhalation of the emanations from the blood and evacuations of cholera patients have been followed by no evil results; and the attempts of the most varied kind which have been made to impart the disease by inoculation and by introducing its different products in every way into the system have been altogether without success.

Dr. Koch made experiments of feeding mice and other animals "on the dejecta of cholera patients and the contents of the intestines of cholera corpses. Although these experiments were constantly repeated with material from fresh cholera cases, our mice," he says, "remained healthy. We then made experiments on monkeys, cats, poultry, dogs, and various other animals that we were able to get hold of; but we were never able to arrive at any-

* *Marseille Médical*, July 30, 1884, p. 402. † *Ibid.* Feb. 28, 1885, p. 94.

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thing in animals similar to the cholera process."* In 1884 the Cholera Commission at Marseilles made an exhaustive series of experiments of a similar kind on animals, chiefly rabbits. The reporter of the results of the Commission, Dr. Livon, reported as follows:—

"The experiments were made with matters ejected from the stomachs and bowels of different cholera patients. The results were all negative, although we varied as much as possible the mode of introducing the matters into the organism. They were injected into the peritoneum, the intestine, the stomach, the cellular tissue, the windpipe, the circulatory system, and always with the same result. The rabbits, animals peculiarly sensitive to operations, resisted their effects thoroughly well; indeed, some of them were inoculated several times without experiencing, apparently, any evil consequences." Though "the histological examination of the contents of the intestines thoroughly demonstrated the existence in them of the comma bacillus of Koch, not one of the animals experimented on presented the least symptom, either clinical or anatomo-pathological, of cholera."

The Commission having made similar experiments with dogs and guinea-pigs, and having ascertained that each of the animals continued in good health, finally arrived at the conclusion expressed by its reporter as follows: "We, therefore, do not recognize in the comma-bacilli any pathogenic influence exerted either, directly, by themselves or, indirectly, by their production of poisonous agents."

The significance of these negative results derives especial importance from the fact that animals do suffer from cholera. Various animals, including dogs, cats, hares, birds of different kinds, and even fish thus suffered during an epidemic of cholera in Austria. It is expressly stated that the disorder affecting the animals was "too generally spread, and too closely connected with the epidemic character of cholera, to admit of its being traced to accidental causes, or its being considered unconnected with the prevailing constitution." When, in 1832–3, cholera was epidemic in Edinburgh, horses and cattle suffered from diarrhœa and cramps; their blood became viscid, and the *post-mortem* appearances were similar to those observed in man. Dr. Sicard, of Marseilles, recounts that during the epidemics of cholera which ravaged that city in 1835, 1849, 1854, and 1855, the birds, and especially the

* Since Dr. Koch made this statement he has convinced himself that cholera can be produced in dogs and guinea-pigs by injecting directly into the small intestines of these animals the comma-bacilli, taken either directly from the choleraic evacuations or from artificial cultivations. We should be very much surprised if these operations did not produce on the animals operated on a state of the stomach and bowels, resulting in vomiting and purging, and even something like collapse—symptoms present in many disorders besides cholera.

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domestic fowls, succumbed in great numbers to the disease.* There is no difficulty in understanding why domestic fowls remained and became victims to the disease: but, as other birds generally leave places where cholera prevails, it is not easy to understand why they did not leave Marseilles. At Lucknow, in 1872, the horses of the 19th Bengal Native Cavalry suffered in the same way, and Surgeon-General J. Murray states that during a cholera epidemic at Agra in 1864, his dog suffered from the rice-water evacuations characteristic of cholera, with which he is convinced the dog was affected. Birds suffer most †—a fact in interesting accord with, and alone explicable by, my hypothesis; for the circulation of the blood in them being more active, and the blood itself being more highly oxidized than it is in any other class of vertebrated animals, their nervous systems are proportionately energetic and impressionable. Probably the especially great susceptibility of the nervous system of birds affords the true explanation of the often observed fact that they leave places where cholera is epidemic. This observation is also applicable to insects, and, above all, to flying insects: their remarkable respiratory system effects the oxidation of their blood to an intense degree, and thus renders their nervous system wonderfully acute; hence it is, I presume, that "flies," as well as birds, have often been known to absent themselves from a place while it was the focus of epidemic cholera.

Dr. T. R. Lewis and Dr. D. Cunningham, medical officers attached to the Sanitary Commission in India, conducted researches for the discovery of a specific organism in connection with cholera. Their inquiries and experiments, extending over a long period, afforded no evidence in favour of "the existence of a specific poison contained in cholera excreta peculiar to them alone, and giving rise to special phenomena." The researches conducted jointly by these two physicians until 1878, were continued afterwards by Dr. Cunningham alone; and in a paper "On the Development of Certain Microscopic Organisms occurring in the Intestinal Canal," he "shows that microscopic parasitic organisms are by no means uncommon in excrementitious matter; that they increase in numbers under known conditions; that certain parasitic forms are specially associated with particular forms of disease, *without holding any causal relation to them.*"

Messrs. Roux and Straus, who in 1883 were sent to Egypt by the French Government to investigate the cause of cholera and who afterwards went to Toulon for the same purpose

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* *Marseille Médical*, Oct. 30, 1884, p. 609.

† See "Notes on Cholera in relation to Animals," by Surgeon-General C. A. Gordon, M.D., C.B., in the *Medical Press*, Sept. 10, 1884.

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also found bacilli in the intestines of cholera patients. They report that in cases of very sudden death from the disease, a bacillus, comma-like in shape, is constantly found in the mucus of the small intestine; but, as Dr. Straus himself stated to the Société de Biologie on the 19th of August, 1884, it is impossible to affirm that it is the cause of cholera, for it has been found in London water, in the dejections of dysenteric patients, and in the mucoid secretions of women enjoying ordinary health.

The comma-shaped bacillus, which Dr. Koch affirms to be the real cause of cholera, has attracted a great deal of attention, but the more attention is given to it by competent observers the more decisively Dr. Koch's doctrine concerning it becomes discredited. He says that in all persons suffering from Asiatic cholera, there occur in the rice-water stools during the acute stage of the disease certain well-characterized bacteria, which, on account of their curved shape, he called "comma-bacilli;" that the comma-bacilli occur in great numbers in the mucous flakes as well as in the fluid of the cholera-evacuations; that they occur in the lower part of that portion of the intestine called the ileum of persons dead in the acute stage, almost to the exclusion of other bacteria, and in such great numbers that the lower part of the ileum may be considered to contain almost a "pure cultivation" of comma-bacilli; that the mucous membrane of the ileum, particularly that of the lower part, around and in the lymphatic glands located there, exhibits in typical and rapidly fatal cases characteristic alterations; that these alterations are due to the presence, growth, and multiplication of the comma-bacilli in these tissues; that the disease, cholera, is caused by the production on the part of the comma-bacilli, and by the absorption on the part of the system, of a special chemical ferment; and that, while comma-bacilli are of constant occurrence in the intestines of patients suffering from Asiatic cholera, they are absent in all other diseases of the intestines.

An inquiry into the etiology of Asiatic cholera has been recently undertaken, by the request and at the expense of the Secretary of State for India, by a Commission consisting of Dr. Klein, Dr. Gibbes, and Mr. Alfred Lingard. After the return of the Commission from India, the chief results of the inquiry were communicated to the Royal Society at its meeting February 5, 1885. By way of introduction to his communication, Dr. Klein presented a summary of the conclusions arrived at by Dr. Koch as the result of the extensive inquiry, during 1883–84, into the etiology of cholera undertaken by him and Drs. Gaffy and Fisher by request of the German Government. It is from this summary that the foregoing expression of Dr.

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Koch's main conclusions is extracted. I now subjoin the principal conclusions of the English investigators.

"The comma-bacilli occur in the mucous flakes of the rice-water stools, as well as in those taken from the ileum of a person dead of cholera. The sooner after death the examination is made the fewer comma-bacilli are found in the mucous flakes; even in typical rapidly fatal cases, the mucous flakes taken from the ileum and examined soon after death (from between fourteen minutes to an hour or an hour and a half) contain the comma-bacilli only very sparingly indeed, *and not to the exclusion of other bacteria.* Our investigations do not bear out Koch's statement as to the lower part of the ileum being in acute typical cases of cholera 'almost a pure cultivation of comma-bacilli.' In not one of the many *post-mortem* examinations of typical acute cases have we found such a state.

"The mucous membrane of the ileum in typical rapidly fatal cases, if examined soon after death, does not contain in any part any trace of a comma-bacillus or any other bacteria, not even in the superficial loosened epithelium. If the *post-mortem* examination be sufficiently delayed, comma-bacilli and other bacteria may be found penetrating into the spaces of the mucous membrane. *Koch's theory as to the comma-bacilli present in the mucous membrane secreting a chemical poison inducing the disease cannot, therefore, be correct.*

"Koch overlooked the fact that '*comma-bacilli*' *occur in other intestinal diseases,* in the mouths of healthy persons, and, as shown recently, even in some common articles of food (by Dr. Deneke in stale cheese).

"The experiments performed by Koch and others on animals do not in the least prove that the comma-bacilli are capable of producing cholera or any other disease.

"There is direct evidence to show that water contaminated with choleraic evacuations, and containing, of course, the comma-bacilli, when used for domestic purposes, including drinking, by a large number of persons did not, in the case of the tanks near the Jelapara Lane, produce cholera."

Another observer not less competent and authoritative than Dr. Klein—viz., Surgeon-Major T. R. Lewis, who, as mentioned above, has worked at the subject with great assiduity for a long period, and whose observations were made during several years in India, and in 1884 at Marseilles—referring to the various microscopic organisms found in choleraic dejecta, one kind predominating in some cases, and another in others, says, "therefore, the selection of the comma-shaped bacilli as the *materies morbi* of cholera appears to be entirely arbitrary." And in his valuable paper on the comma-shaped bacillus, published in 1884, from

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which these words are quoted, he makes the important statement, repeated by Dr. Klein, a statement which seems to me absolutely fatal to Dr. Koch's theory:*—"Comma-like bacilli, identical in size, form, and in their reaction with aniline dyes, with those found in choleraic dejecta, are ordinarily present in the mouth of perfectly healthy persons."†

In the *Lancet*, January 31, 1885, there appeared an extremely interesting, important, and suggestive paper entitled "The Comma-shaped Bacillus: a Zymogenic, not Pathogenic, Entity," and signed, George Waters, Surgeon-Major I.M.D., Professor of Pathology, Grant Medical College, Bombay. He states that, having "set to work in the microscopic examination of mucus from the small intestines of persons who had died from ordinary affections," he "found curved bacilli, identical in shape, size, and appearance with comma-bacilli, in all or nearly all;" and that though "these bacilli are to be found much more abundantly in the *flaky* mucus so commonly met with in the ileum after death from cholera than elsewhere in the digestive tract or in this part of the bowels, under other circumstances," yet "the *ordinary* mucus even of cholera shows not many more bacilli than are to be seen in the mucus of the small intestines of persons who have died from other causes."

Professor Waters also says:—

"In human urine I found curved bacilli within five minutes after the urine was voided from the bladder. Some six hours later the same urine showed a slight increase in these organisms; and this slow multiplication goes on till the urine becomes neutral or slightly alkaline, when they are to be seen in numbers such as are only elsewhere to be found in cultivations from the flaky mucus already mentioned. . . . . It would appear, then, that curved bacilli are not specifically connected with cholera. . . . . They are, nevertheless, an indubitable pathological feature of that disease; but this, I think, will soon be shown to be something more in the shape of a consequence than a cause of the malady."

Indeed, the more the subject is studied, and the more well-authenticated facts connected with it accumulate, the more the

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* "In reality, it seems to be proved that Dr. Koch's discovery of the comma-bacillus, as the cause of cholera was anticipated by Messrs. Brittan and Swayne, who, in 1849, published in the *Provincial Medical and Surgical Journal* a detailed description of a micro-organism which they found in the dejecta of choleraic patients, and which resembles in a striking manner the comma-ferment."—*Bulletin Mensuel du Bureau de Démographie et de Statistique de la Ville de Marseille*, Mars, 1885, p. 240.

† A Memorandum on the "Comma-shaped Bacillus," alleged to be the cause of Cholera. By Surgeon-Major Timothy Richards Lewis, M.B., Assistant-Professor of Pathology, Army Medical School. This masterly and important Memorandum was published in the *Lancet*, September 20, 1884.

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conviction strengthens that there is no causal relation between comma-bacilli and the development of cholera, that they are in no sense whatever agents or media of its diffusion, that their discovery affords no support to the doctrine of its contagiousness, and that the etiology of the disease propounded by Dr. Koch, which, creating in the imagination of the timid countless swarms of invisible, death-diffusing, atoms against which defence is impossible, has held Europe panic-stricken for months, is after all but "the baseless fabric of a dream."

If, as now demonstrated, the comma-shaped bacillus is not the cause of cholera, two questions arise concerning it—viz., What function does it really fulfil in the human organism? and, Why is it more abundant in the small intestines of persons suffering from cholera than it is in those of sufferers from other diseases? To the first of these questions Professor Waters has suggested an answer which is original and especially ingenious, and, if not absolutely true, is one of those "guesses at truth" which seem to approach very near it. To the second question I shall attempt to give an answer which, if not found wholly satisfactory, will, I hope, at least throw some light on the subject.

After stating that comma-shaped bacilli "can be seen by any one in healthy urine and in the mucus of the small intestines of those who have died from ordinary diseases, and are thus presumably discoverable throughout the mucous tract under most circumstances," Professor Waters says:—

"This leads me to the expression of an opinion that has been gaining ground in my mind for some time, which is that the organism in question should be regarded more as a physiological than as a pathogenic entity. Dr. Koch's own careful observations lend a measure of support to this supposition. He noticed that a colony of comma-bacilli in cultivation to a certain extent dissolved the sterilized proteids on which they were nourished; *presumably, the proteids were thus converted into peptones.* He noticed also that for some days these organisms multiplied to the exclusion of all others in the flaky mucus of cholera kept on linen in a moist atmosphere, and then gave place to those characteristic of putrefaction. Now this is exactly what I find in the case of urine; until it becomes markedly alkaline, or perhaps I should say till decomposition commences, it shows myriads of curved bacilli, to the exclusion of all other organisms; whilst beyond this point large straight putrefactive bacteria predominate. Indeed, the former are seldom to be found in decomposing urine. This would appear to indicate the small comma-bacilli to be *fermentative*, and the large straight bacilli putrefactive, organisms. . . . . Now we either know or suspect that fermentation plays a part in the digestion which takes place in the lower portion of the small intestine; but hitherto we have failed clearly to identify the agency by which it is accomplished. And here, I think, some light has been shed by Dr. Koch's observation that

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growing colonies of comma-bacilli liquefied in their immediate neighbourhood the proteids on which they grew. We have it on good authority that the bacillus butyricus is commonly found in the stomach and intestine of herbivora, and the fact that this organism has the property of decomposing cellulose at once explains its importance in the digestive process of such animals. Why, then, should a similar zymogenic function not be attributed to an organism like the comma-shaped bacillus, which seems to have its normal habitat in the presence of mucous membrane in the human body? Indeed, I should not be surprised to find it conclusively proved that *the comma-shaped bacillus is specially concerned in nitrogenous fermentation.*"

Thus it seems possible that this microscopic entity, which under Dr. Koch's management played the part during 1883–84 of the plague demon incarnate, and spread terror throughout Europe and even America, is in reality an exclusively good genius—displaying its especial beneficence to man by occupying itself incessantly in promoting and completing the all-important function of digestion!

If Professor Waters' theory be in the main correct, and it must be admitted to have a scientific aspect, at least one explanation is needed before it can be accepted without reserve—viz., why, if, as a fermentative agent, the function of the comma-bacillus is digestive, it nevertheless presents itself in fluids (the urine and fluor albus, for example) in which no digestion occurs. He will perhaps reply that Nature acts on general principles, and is as a rule excessively exuberant; that it appears to be a function of mucous membranes to produce comma-bacilli; that where they are useful, as in the alimentary canal, they are utilized; but that where they are seemingly useless, as in the fluids just mentioned, they are simply cast away with that reckless prodigality and destruction of life which Nature everywhere displays.

Why comma-bacilli are more abundant in the small intestines of choleraic patients than in those of sufferers from other diseases is interesting in itself, and especially important in its relation to the etiology of cholera which I have propounded. Professor Waters touches on this question in the following words:—

"If I say that a superabundance of comma-shaped bacilli is found in the small intestines of cholera victims, because it would appear that whatever gives rise to that ailment brings about exalted fermentation in that part of the alimentary canal, I shall, at least, have as much logic in my assertion as Dr. Koch had in declaring that these organisms must bear a causal relation to the disease."

But the question, how that condition in the small intestine designated by Professor Waters "exalted fermentation" is produced, still remains to be answered. I may say, *in limine*,

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that, in my opinion, that condition would be more appropriately designated by the term "exalted vitality," and this opinion will probably appear to receive confirmation by the considerations I am about to adduce. At page 12 of this essay I have quoted the statement of Dr. George Johnson that "the flocculi in the rice-water stools consist almost entirely of *perfectly organized epithelial cells, most of them of large size;*" that "the peculiar creamy viscid secretion, which sometimes nearly fills the small intestines after death, is *almost entirely made up of the same fully formed epithelium;*" and that "*this abundant cell-formation can result only from a very active vital effort.*" Sir Andrew Clark favoured me in 1866 with the results of his microscopic examination at the London Hospital of the small intestines of victims to cholera in that year. I have given his communication *in extenso* at p. 85 of my work "Diarrhœa and Cholera," and quote here the part of it having special reference to the question at issue. He says, "Each [mucous] flake consisted of a basis of mucin, sometimes slightly fibrillated, and having imbedded in it innumerable granules *in active movement; cells resembling the colourless corpuscles of the blood; larger spherical nucleated cells* in different stages of development and decay, varying in size from 1-1500th to 1-8000th of an inch in diameter; compound granule cells; blood disks; and fragments of vegetable tissue." These facts, authenticated by two especially competent observers, show conclusively that the proliferation of cells by the alimentary mucous membrane in cases of cholera is extraordinarily great; and this conclusion is decisively confirmed by *post-mortem* observations of the state of that membrane itself. That of the stomach is usually thickened and opaque, its texture firm, its surface mammillated, its solitary glands enlarged. The coats of the small intestines are thickened and pulpy; their glands are swollen and enlarged, and are therefore much more prominent and distinct than normal. "The most frequent of all abnormal conditions of the mucous membranes," says Dr. Gairdner, "is the prominence of the intestinal glands, both of the aggregated and solitary, but especially of the latter." The changes observable in the large intestine are of a like kind, but less in degree. In many instances the solitary glands of this intestine are especially distinct: they are round, more or less prominent elevations, sometimes surrounded by a circle of injected vessels, and often translucent from distension of the walls of their cavities. A like condition in the female reproductive organs has been noted by several observers. Virchow reports having found "in the uterus great swelling and hyperæmia of the mucous membrane, with enlargement of the uterine glands." It is stated that "in all females," at the London Hospital in 1866, "there was a

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muco-purulent discharge from the vulva, which in adults was stained with blood, even though it was not the menstrual period."

In instructive accord with this excessively exuberant activity of the mucous membrane characteristic of the collapse stage of cholera is the thoroughly ascertained fact that the temperature of the bowel, as denoted by that of the rectum, is abnormally high: "Experiments in Paris," says Dr. Macpherson, "show that the temperature rises to about 103° F. in the rectum." Dr. Weber reports several cases in which he found the temperature in the rectum higher than normal, and "one case of severe collapse" in which the temperature was 103° F. Mr. Mackenzie's investigations at the London Hospital during the cholera epidemic of 1866 proved that the temperature in the rectum of patients in collapse ranged ordinarily from 101° to 103° F. In cases in which death occurs in the algide stage, "the temperatures in the vagina and rectum sometimes reach still higher degrees (40° Cent. = 104° Fahr., or more, and in one case of Güterbock's, even 42·4, Cent. = 108·32° Fahr.).* Sir John Simon may well conclude as he is reported to have done, that "the choleraic affection of the bowels is a heat-making or inflammatory" process. This phrase is only another version of Professor Waters' description, "exalted fermentation," and, as already mentioned, I prefer to either of them the term "exalted vitality."

Now it is well known that each of those glands, the innervation of which has been ascertained anatomically, secretes only when it is stimulated to do so by nerve force emanating from the cerebro-spinal nervous system; and Bernard inferred, chiefly from the facts discovered by himself in respect to certain glands, that all glands or secreting structures function only at the bidding of the particular nervous centres anatomically related to them. By my experiments and observations I have been able to prove that his inference is correct, for I have discovered, and have verified the reality of the discovery over and over again, that the function of the glandular system generally, and notably that of the mucous membrane of the stomach and bowels, may be increased by the application of heat along the spine, and may be decreased by cold similarly applied. It is evident, therefore, that the extremely prolific activity of the whole glandular system of the alimentary canal, as denoted by the extraordinarily exuberant secretion of mucus, the rapid development and shedding of cells, and the increased size and prominence of the intestinal glands generally, which are characteristic phenomena of the algide stage of cholera,

* "On the Temperature in Diseases," by Dr. C. A. Wunderlich. Translated by W. B. Woodman, M.D. London, 1871.

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is exclusively due to the preternaturally energetic nervous stimulation which that system receives during this stage.

What exactly is the process by which the comma-bacillus, as one of the products of the mucous membrane, is primarily evolved is not yet ascertained; but Dr. Klein's observations have already thrown a good deal of light on the subject. He says:—

"The mucous flakes taken from the small intestines of a typical rapidly fatal case of cholera contain numerous mucus-corpuscles filled with peculiar minute straight bacilli; in this state they are found when the examination is made very soon after death; soon, however, the mucus-corpuscles swell up and disintegrate, and then their bacilli become free. They are one-third, or one-fourth, the length of the comma-bacilli, and about half their thickness. . . . . The comma-bacilli of cholera show two distinct modes of division, one the known one of transverse division, and a second one of division in length. When growing in Agar-Agar jelly at the ordinary temperature of the room, after some days the bacilli swell up, owing to the appearance in their protoplasm of one or more vacuoles; as these vacuoles increase, so the comma-bacilli become gradually changed, first into plano-convex, then into oblong bi-convex, and ultimately into circular corpuscles. These circular organisms are mobile, just as are the comma-bacilli; and, by disintegration of the protoplasm at two opposite points, two perfect, more or less semicircular comma-bacilli are formed."

There is still a missing link in respect to the primary generation of the comma-bacillus; but inasmuch as the small straight bacillus is evolved from a mucus-corpuscle, and as one of the modes of multiplication of the comma-bacillus is by means of a corpuscle, the discovery of the mode of *origin* of the latter is scarcely likely to be far off. In this connection I may mention that Drs. Nicati and Riesch, of Marseilles, describing the results of their post-mortem examinations in cases of cholera, observe:—"When the patients have succumbed rapidly, the intestine, from the pylorus to the rectum, is gorged with the characteristic rice-water. This consists of albuminous serum loaded with epithelial cells and a certain number of bacilli: *at the end of twenty-four hours the epithelial cells disappear, they are literally replaced by comma-bacilli.*" *

It seems, then, that in the present state of our knowledge of the life-history and doings of the comma-bacillus we are enabled to affirm (1) that it is produced from the human mucous membrane of the mouth, of the intestines, and of the genito-urinary organs; (2) that, inasmuch as it has been found in the mouths of healthy persons and in the intestines of persons whose deaths were not

* *Marseille Médical*, Oct. 30, 1884, p. 614.

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due to enteric disease, it is probably a normal product of the human mucous membrane; (3) that, noting its power of dissolving proteid compounds, and noting the fact that the bacillus butyricus found in the alimentary canal of the herbivora has the power of dissolving cellulose, we see that each of these two kinds of bacillus seems to throw light on the nature and functions of its analogue, and are thus entitled to accept the conclusion of Professor Waters —viz., that the comma-bacillus is a co-operative agent for the completion of the digestive process; (4) that, being a normal product of the human mucous membrane, its presence in abnormally large numbers in the small intestines of persons suffering from cholera is completely accounted for by the now thoroughly established fact that during the algide stage of cholera the mucous membrane of those intestines is excessively active—the development of its gland cells, which are shed in quick succession, being extraordinarily exuberant; hence the production of comma-bacilli in excessive numbers is only an incident and consequence of the generally exalted vitality of that membrane. The excessive abundance of them in choleraic patients is clearly, therefore, in no sense the cause of cholera, but only one of the many consequences of that violently excited and over-active condition of the nervous system which is, in fact, the immediate cause of cholera.

After the preceding paragraph was written and sent to the printers, I had the great satisfaction of reading the two important communications by Dr. Klein, which were published in the *British Medical Journal* for May 9 and 16, 1885, respectively, as well as the reports, published in the *Marseille Médical*, of the physiological researches concerning cholera recently conducted in Marseilles, and which confirm in the most decisive manner the views summarized in the above paragraph. Dr. Klein writes:—

"In an article on the so-called cholera-bacilli, published in the *British Medical Journal* for May 2, 1885, Mr. Watson Cheyne says, on page 878: 'One of the most peculiar forms which I have seen was found in the contents of the large intestine of the guinea-pigs, which died after injection of cholera-bacilli' (killed by the injection of cholera-bacilli into the duodenum). 'I tested the fluid by cultivation at the time very carefully, and found that it contained almost a pure cultivation of cholera-bacilli; there was certainly not more than one other kind of bacterium for every 100 cholera-bacilli. The appearance of this material, on microscopic examination, after staining, is shown in the accompanying figure (see Fig. 6). Large, fat, coiled, almost worm-like organisms will be seen, which, as I know by cultivation, are cholera-bacilli . . . . .'

"Upon these statements of Mr. Watson Cheyne, I wish to make the following comment. Any one desirous to see and examine these comma-bacilli ('cholera-bacilli') need not wait for 'guinea-pigs, which died after injection of cholera-bacilli.' If he have a perfectly normal

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half grown or adult guinea-pig killed, that has not been the subject of any experiment whatever; and if he will take the trouble of examining the contents of its cæcum, he will there find crowds of the very identical 'cholera-bacilli,' accurately represented by Mr. Cheyne in his Fig. 6, as well as the typical comma-bacilli of Koch.

"In some places, in the stained cover-glass specimens, the material thus obtained appears to be 'almost a pure cultivation' of them."

The comma-bacillus is evidently becoming a very common bacillus. It has been found in stale cheese (by Dr. Deneke); in London water (by Dr. Maddox); in the Seine (by Dr. d'Héricourt); in the water at Marseilles from the source called "La Rose"* (Marseilles Cholera Commission †); in guinea-pigs (by Mr. Cheyne and Dr. Klein); in monkeys, as we shall presently see (by Professor Horsley and Dr. Klein); in the mouths of healthy persons (by Dr. Timothy Lewis of Netley, and by Professor W. D. Miller of Berlin); in the vaginal mucus of women suffering from leucorrhœa or epithelioma (by Dr. Straus); in the dejecta of patients suffering from chronic dysentery (by M. Malassez‡); in the dejecta of patients suffering from "cholera nostras" (by Finkler and Prior of Bonn); "in the mucus of the small intestines of persons who have died from other causes" than cholera (by Professor Waters); and, finally, in human urine (also by Professor Waters).

Now it appears that the comma-bacilli found under the various conditions here enumerated are, so far as microscopic examination can determine the question, identical with each other in form and nature. But Mr. Cheyne, the able and valiant English advocate of Dr. Koch's doctrine, and Professor

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* La Rose is a village about five kilomètres from Marseilles. Near the village is a spring of the same name. The water from this source is conveyed to Marseilles, and is used by its inhabitants.

† "Dr. Koch having acknowledged the contamination of water by the bacilli, we examined the canal water, taken from the laboratory tap, and water taken from the Rose near to its source. Both contain them. Desirous of ascertaining the number of comma-microbes contained in a litre of the Rose water, we made a series of observations, following exactly the method indicated by Koch. We found that each drop contains on an average ten comma-bacilli (twenty-five drops to the cubic centimetre). This represents a number of 250,000 comma-microbes to the litre, without counting other kinds of microbes also found in the water. Dr. Koch declared, at the conference held at the Pharo Hospital, that a single microbe suffices to kill a man! It should be noted that these observations were made with a magnifying power of 600 diameters, and that these bacilli, compared with the bacilli figured by Koch, presented no difference, in either aspect, or dimension, or colour. Now that the epidemic is drawing to a close, and although at La Rose no case of cholera has occurred, we have considered ourselves bound to make a comparative examination and affirm that the 250,000 comma-microbes are still to be found." —*Marseille Médical*, Oct. 30, 1884, p. 579.

‡ Cited by Dr. Straus in *Le Progrès Médical*, 4 Avril, 1885.

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Miller of Berlin, as well as other pathologists, maintain that though these various bacilli are identical in appearance, they are not in nature, and that the comma-bacilli of Koch has distinctive characteristics by which it is differentiated from all other comma-bacilli. They base their assertion on the fact that when the latter are subjected to the process of "artificial cultivation," their behaviour is different from that of the true "cholera-bacillus" of Koch.

Now it seems to me (and I venture to make the suggestion with the utmost deference to the eminent microscopists and pathologists who are engaged much more closely in the strife than I am), that if the behaviour of these several bacilli obtained from various and very different sources did not differ from that of Koch's bacillus when subjected to precisely the same method of "cultivation," the fact would be very surprising indeed. A comma-bacillus bred in and fattened on cheese, and one produced in the lower part of the human intestine, may so resemble each other in appearance as to be indistinguishable; but they must need, I imagine, to be elaborately "educated" before they can exactly resemble each other in manners and general deportment. Probably, however, appropriate "cultivation" during two or three generations, would render them *in all respects* identical.

The lining membrane of the human mouth, together with that of its salivary ducts, the lining membrane of the small intestines in man, in monkeys and in guinea-pigs, the lining membrane of the womb and vagina, and the lining membrane of the bladder are each and all mucous membranes, and are each and all producers of comma-bacilli exactly like each other; but if we duly consider the widely different quality of the different fluids having mucus as their basis, which are in constant contact with these different membranes, we can scarcely escape the conclusion that, though the source and mode of origin of the bacilli which they severally produce are analogous, the bacilli evolved from each special membrane must partake of its special character, and therefore are not likely, in so far as they do so, to manifest identical characteristics when they are submitted to one and the same process of "cultivation." But if, when submitted to this test, they do not manifest identical characteristics, can this negative result avail as an argument in proof of the truth of Dr. Koch's contention that the comma-bacilli found in the intestines and dejecta of sufferers from cholera are the cause of the disease? If these bacilli are so appallingly destructive of human life, as Dr. Koch alleges them to be, how are we to account for the wonderful fact that their kindred, identical with them at least in form, and also bred in, as well as inhabiting,

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mucous membranes are so wholly harmless as they appear to be? If the bacillus found in the intestines of patients, who have died of so-called "Asiatic" cholera, be the cause of the disease, by what process of logic can Dr. Koch and his disciples maintain that the bacillus found in the dejecta of patients suffering from "cholera nostras" is not the cause of this milder form of cholera? Such a fine distinction as this seems to me to be much too fine to be tenable.* I prefer, therefore, in the present state of our knowledge on the subject, to follow the lead of common sense, which evidently indicates that the bacilli detected in the several and different conditions above mentioned have a common origin from mucous membrane, that the slight modifications from each other which they manifest when subject to "cultivation" are due to the differences of their original surroundings—saliva, for example, in one case, fæcal matter in another, and urine in another—and that as they are all alike in origin as well as in form, and as all but one of them are admitted to be harmless, the presumption is thoroughly justifiable that the notorious bacillus of Dr. Koch is harmless also †—the special abundance of it in cases of algide cholera being due, as affirmed above, to the morbidly excessive activity of the mucous membrane—the common parent of all comma-bacilli in the human body, whatever may be the locality in which they are found.

I have already stated that I had read the two last communications of Dr. Klein with especial satisfaction; the second of the two, which I am now about to quote, gave me especial pleasure, because, though he has not yet adopted my hypothesis

* Referring to bacilli taken from the dejecta in question, Dr. Straus says: "Morphologiquement il nous a été impossible de trouver, entre l'organisme de MM. Finkler et Prior et le bacille virgule, la moindre différence appréciable. D'après M. Koch l'aspect des cultures serait different, et l'organisme trouvé par les médecins de Bonn se développerait plus rapidement et fluidifierait plus vite la gélatine. *J'avoue que, dans nos culture comparatives, les mêmes differences ne nous ont point frappés, et que même nous avons fait une constatation inverse, l'organisme de Finkler nous paraissant se developper moins vite que le bacille virgule de provenance asiatique.* Quoi qu'il en soit, les différences ne reposent que sur des nuances de cultures et cette constatation de deux organismes presque identiques dans les selles de choléra indien et dans le choléra nostras est bien remarquable."—*Leçons sur l'anatomie pathologique du choléra*, par le Dr. J. Straus, agrégé, médecin de l'hôpital Tenon: *Le Progrès Médical*, 4 Avril, 1885.

† In a letter addressed to me (June 15, 1885) by Dr. Poucel, of Marseilles, he says: "Of the series of rabbits subjected to the experiment of injecting bacilli, taken from the intestines of choleraic patients, into the duodenum (without previous ligature of the bile-duct), I had some which survived ten and eleven days. Now, these rabbits never presented any choleraic symptom; and, at the *post-mortem* examinations made by Monsieur Taxis and myself, we, nevertheless, found a multiplication of comma-bacilli far more abundant than is ever found in choleraic patients."

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concerning the origin of the comma-bacillus in man, the fact he describes is all that was still needed to transform that hypothesis into simple truth; and because he is evidently beginning to look with favour on the doctrine that "the state of the intestine" in cholera is produced by nervous agency. The following is the important communication in question:—

"Let us suppose that the comma-bacilli—although present in the normal intestine, but in exceedingly small numbers, too small to be capable of demonstration—require, for their appearance in numbers, that is, for a rapid multiplication, some pathological condition of the intestine, such, for instance, as obtains in cholera, this condition being favourable for their growth and multiplication, perhaps more favourable to them than to the other bacteria. Now, if we could produce such a state of the intestine, and if hereby we could create those conditions favourable for the growth and multiplication of the comma-bacilli, then we might expect to find the comma-bacilli in numbers sufficiently great to demonstrate their presence with comparative facility, both by microscopic examination and by cultivation.

"We do not know, it is true, beyond the anatomical facts, what the exact nature of the state of the intestine is in cholera, whether it is the result of the action of the virus on the secretory nerves of the intestine, or on the vaso-motor nerves, or directly on the tissue of the intestine; but I have attempted to produce a state which, in anatomical respects at any rate, resembles to a limited degree that obtained in cholera.

"Professor Horsley, of the Brown Institution, has been kind enough to make for me the following experiments. In a monkey that had received a dose of castor-oil the previous evening, the abdomen was opened under the spray, the lower ileum was drawn out, one ligature was tied just above the ileo-cæcal valve, another about two to four inches higher up; the large vessels of the mesenteric border were avoided as much as possible; into this loop a hypodermic syringe, full of a saturated solution of magnesium-sulphate, was injected; the ileum was replaced, the wound stitched up, a bandage was applied, and was covered with collodium. All instruments, threads, &c., had been previously well cleaned and disinfected. In this manner, six monkeys were operated upon; in one instance the animal, after the operation, received subcutaneously one grain of chloral-hydrate in two cubic centimètres of water. One of the animals was dying the same evening, one was dying before three days were over, the others were killed after forty-eight hours.

"Of the mucous contents of the loop of the ileum that had been tied, and before the injection of the magnesium-sulphate, a quantity had been withdrawn by means of a hypodermic syringe, and carefully examined for comma-bacilli, but none could be found. In three of the animals killed after forty-eight hours, the tied loop was found, on *post-mortem* examination, to be much injected; its cavity contained a quantity of a brownish fluid, in which were suspended numerous mucus-flakes; the epithelium of the mucous membrane was in many

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places loosened or detached. In the contents were present undoubted comma-bacilli; particularly in one of the animals (the one that received subcutaneously, after the operation, one grain of chloral-hydrate) the comma-bacilli were present in large numbers—more numerous than I have seen them in some typical acute cases of cholera. In some fields of the microscope, I estimate their number at over 50 per cent. of all the organisms present.

"It is evident from this, that the pathological state of the intestine produced the condition favourable for the multiplication of the comma-bacilli, and that they are, therefore, the *result* and not the *cause* of the disease. These comma-bacilli appear as single commas, as double commas—either S-shaped or placed end to end, their curve in the same direction—and as short spirals. I am unable to distinguish them from the choleraic comma-bacilli, and they appear to be identical with the latter."

In presence of the large array of facts now presented to the reader, proving the harmlessness of the comma-bacillus, he will readily understand how far from dangerous was Dr. Klein's experiment in Calcutta, and M. Rochefontaine's in Paris, of swallowing a number of these renowned microbes.

In the second report of the Marseilles Medical Commission is the following explicit and very decisive statement:—"*The epithelial desquamation* [*of the intestines*] *is a phenomenon which we render prodigiously active to the extent, sometimes, of causing the intestinal contents to become like rice-water by simple lesions of the solar plexus.*" *

If the evidence here presented constitutes an overwhelming and indisputable proof, as I believe it does, that the comma-bacilli "are the *result* and not the cause" of cholera; no argument is needed to prove that inoculation with this microbe, or with any such modification of it in the shape of the "germs" which Dr. Ferrán, of Valencia, claims to have discovered, can avail in any degree as a protection against cholera. I shall abstain, therefore, from discussing either Dr. Ferrán's doctrine or his practice, and am confident that at no distant time the enthusiastic and widespread excitement which they have caused in the Spanish people, will be followed by a recognition that they were suffering from a strong delusion and by a consequent disappointment as great as is the delusion itself.†

I now invite the attention of my readers to an entirely different aspect of the present part of my subject, but one which is

* *Marseille Médical*, Feb. 28, 1885, p. 67.

† Since this paragraph was put in type, Dr. Brouardel's report to the Academy of Medicine, concerning Dr. Ferrán's proceedings, has been published: it wholly justifies the appreciation expressed in the text.

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especially important and peculiarly interesting—viz., the *state of the blood-globules in the algide phase of cholera.*

At pages 18 and 19 of this essay I have adduced facts in support of the statement that the blood of cholera patients is essentially healthy—the only exceptions to the correctness of this statement being that in a certain proportion of cases it contains sugar, and that, inasmuch as during the algide stage the secretion of bile and urine is arrested, bile and the distinctive elements of urine are retained in the blood. Of course, however, the presence of these several matters in the blood does not render it poisonous in the ordinary sense of that term. If blood containing them, but containing no other foreign elements, were injected into the veins of healthy persons, it would exert no poisonous influence.

But the Marseilles Commission of Inquiry concerning cholera, in its Report, read Sept. 13, 1884, declared that it had discovered an alteration in the blood-globules of cholera patients *while in the algide stage*—an alteration which was found in all the cases examined.

"The alteration consists in softening of the globules which consequently become deformed by mutual pressure, and the masses of globules agglutinate, the more so as the period is advanced. Then if there should happen to be a current on the observation-plate, diseased globules may be seen to flow like fluid lava or melted pitch between the more compact masses, and their adherence is such that the globules may be seen to elongate owing to the mechanical effect of the current, and to assume olive-like, almost cylindrical, forms, and to stretch themselves until they become free, and then if the alteration is not advanced they, by virtue of their elasticity, return to their primitive form. But we have ascertained that in very grave cases the globule loses its elasticity and retains the olive-like form even when isolated. We assured ourselves that it was deprived of all adherence to neighbouring globules, even by fibrinous filaments. If during microscopic examination artificial serum be added [to the blood examined], the greater part of the globules may soon be seen to resume their independence, their normal form, and their arrangement like a pile of coins; but wherever the globules are the most altered, there the disaggregation does not take place." *

In a subsequent Report, read Sept. 20, 1884, by Dr. Poucel, he says:—

"The globule lesions are constant in cholera; that is indubitable. They are of very great importance in respect alike both to diagnosis and prognosis. Thus in every case in which softening of the blood-globules goes on to the extent of causing them to lose all elasticity—when, for example, the current produced on the plate under observation

* *Marseille Médical*, Oct. 30, 1884, p. 581.

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preserves its swiftness at one point and is retarded or stopped at another—the intermediate globules being dragged, become lengthened and stretched, then if the adherence is overcome, some globules remain free in the lacuna which results from this rupture; now so long as these globules return to their primitive form the patient may recover; but each of those patients in whom the free globules were observed to remain elongated, olive-like, died." *

The alteration of the blood-globules described above presents a remarkable feature—viz., that it does not attack all the globules simultaneously, but, quite on the contrary, globules profoundly changed may be seen, especially in cases which have a rapid termination, by the side of globules perfectly healthy which assume their normal disposition like a pile of coins.†

Here, at length, seemed to be a discovery of a symptom affording a strong presumption that the blood of cholera patients is poisoned; and if so, would not this blood produce cholera in animals? To answer this question, the Commission performed twenty-eight experiments on animals—chiefly rabbits. Twenty-three of these experiments failed altogether to impair the health of the animals experimented on; but two rabbits injected with blood taken from the *dead body* of a person who had died in the beginning of the algide stage, died at the end of from twelve to eighteen hours, presenting anatomo-pathological lesions which the Commission were inclined to regard as belonging to cholera, as well as the hæmatic alterations already described. Two other rabbits which were submitted to intra-venous injection of blood from a cholera patient at the beginning of the algide stage, were affected the next day with "coloured diarrhœa," and in the blood of one of them agglutinated globules were observed. A dog also operated on in like manner had serous diarrhœa during two days.

With due respect to the scientific procedure of the members of the Commission in question, I feel constrained to pronounce the results of the two experiments with the blood of the cholera corpse worthless; there remain, therefore, three only out of the twenty-eight in which results meriting consideration were produced. In these three cases diarrhœa occurred after the injections, and in one of them agglutination of blood-globules. Now I submit that these results are very far from adequate to sustain the hypothesis put forward by the Commission, that the blood of cholera patients is poisoned and poisonous. Every one knows how very slight in many cases are the causes capable of inducing diarrhœa in human beings; and, considering how peculiarly

* *Marseille Médical*, Feb. 28, 1885, pp. 65–6.
† *Ibid.* Oct. 30, 1884, p. 581.

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sensitive (as insisted on by the Commission) rabbits are, and the effects of fright on the nervous system, the wonder is that an injection of foreign matter (human blood) into their veins did not produce disorder (diarrhœa, for example) in a larger proportion of cases than two out of twenty-five. I venture, therefore, to express a very strong doubt as to the pathological value of the production of diarrhœa in the two rabbits in question; and this doubt is, I think, equally applicable to the result of the experiment on the dog.

It will perhaps be said, "If these experimental results stood alone, the estimate of them just expressed might be considered just; but when they are viewed in connection with the ascertained lesions of the blood-globules in cases of algide cholera, they assume important significance." Possibly. But if the blood of cholera patients be really poisonous and, nevertheless, manifests its virulence in only two rabbits out of twenty-five to which it is administered; if, as in fact has been demonstrated, only a varying proportion of the blood-globules appear diseased—the healthy ones freely intermingling with them; and if, as alleged, it is only poisonous during the early part of the algide stage, and not afterwards, these remarkable peculiarities are, it must be admitted, three more added to the already great number of wonderful and contradictory characteristics of cholera, the nature of which is still held to be inscrutable.

As page 18 of this essay I have intimated that, in the opinion of a large number of competent observers, whatever differences in the constitution of the blood of choleraic patients dying in collapse from that of normal blood are discoverable, are differences mainly due to the withdrawal from it of a large amount of its most fluid portion; and, according to the microscopic observations of the Marseilles investigators, it appears that when artificial serum is added to the "sick" globules under observation, the greater part of them may be seen quickly to resume their independence, their normal form, and their disposition resembling a pile of coins. These results seem to me to intimate that the apparent sickness of the globules which is supposed to be induced by a hypothetical poison, is really due, mainly at all events, to the fact that the quantity of the medium in which they live has been greatly lessened, as well as chemically modified, owing to insufficient oxygenation, and, therefore, that the vital changes which normally take place in them and effect their nutrition and development, are more or less completely arrested. But though many of the sick globules, presumably those which are least so, seemingly recover their normal state when treated by the addition of artificial serum, a certain proportion of them are not thus recovered. They are probably the least robust

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(perhaps the youngest and the oldest) at the onset of the disease, while those which remain unaffected are the most vigorous of all. But if the morbid phenomena in question are the result of a poison, how are we to account for the fact that a large proportion of the sick globules are recovered by the mere addition of serum to that in which they swim, and that many of the others show no signs of sickness at all?

It appears that the morbid condition of the blood-globules characteristic of algide cholera is the result of two factors, one hydro-dynamic and the other neuro-dynamic. The hydro-dynamic factor consists in the withdrawal from the blood of a large proportion of its serum; and the proof that it does so has already been adduced—viz., the authentic statement that if artificial serum be added to the morbidly affected globules a large proportion resume their normal form and aspect. The neuro-dynamic factor consists in violent perturbations of the nervous centres—perturbations transmitted through nervous agency to, and made manifest in, the blood-globules; and the proof that it does so is afforded by recent experiments which demonstrate that a condition of the blood-globules, precisely like the one in question, can be produced in healthy animals by acting on the nervous system. At Marseilles, a Commission, supplementary to the one whose labours I have already availed myself of, and consisting of Drs. Sicard, Poucel, and Taxis, set themselves to determine whether it is possible to produce the globule-lesions in question experimentally in a healthy organism.

"In order to realize these modifications of the general circulation we have made numerous experiments which were inspired by the results of the researches of Ludwig and Cyon, who have established that there exists a constant but inverse relation between the tension of the blood in the abdomen on the one hand, and that of the blood of the general circulation on the other; so that in fact every vessel-dilating action exerted by the solar plexus is followed by general anæmia, whereas its vessel-constricting action chases the blood from the abdomen to the other organs."

Those of my readers who wish to know in detail the character of the experiments performed on the nervous system under the inspiration just mentioned, are referred to the Report of the Commission in question. Here I must content myself with reproducing, in the words of the reporter of that Commission—Dr. Poucel—the general conclusion at which it arrived. In pursuing their inquiries they sacrificed a series of rabbits, and the reporter says:—

"We have been able to establish that in all the rabbits, according to the quantity of serum, and according to the surrounding temperature, *the globules undergo an alteration absolutely like that observable*

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in cholera, at the end of from thirty to forty minutes in the supra-renal capsules, at the end of from eight to twelve hours in the liver and in the cortical layer of the kidney. We have been able to ascertain that both in dogs and rabbits the same softening or fluidifying of the hæmoglobin may be affected by retarding or obstructing the capillary circulation in living tissues, or even by mixing with the normal blood a portion of the supra-renal capsule. It became, therefore, evident that the phenomenon of softening and dissolution of the hæmoglobin is a phenomenon which may be produced in healthy organs and, perhaps, in every living cellular element, but above all in those which perform a special hæmo-poiétic function."

These investigators concluded that

"the globule lesions are not due to the passage into the blood of this or that normal, excrementitial, or re-crementitial principle, and that cholera is not the result of a poisoning by diastase, bile, pancreatic juice, or by their salts or ferments."

These gentlemen, referring to the fact that the globules are not all morbidly affected at the same time, and that those which are affected are so in different degrees, express the opinion that the alteration "takes place in this or that organ, and becomes general in proportion as fresh quantities of blood remain imprisoned the necessary length of time in these organs, owing to the capillary obstruction" which has occurred.

The final conclusion of the Commission is expressed in the following words:—"We are led to regard the anatomo-pathological lesions of cholera—*including the lesions of the blood-globules*—as solely dependent on troubles of cardio-vascular innervation. These troubles are, according to us, the direct result of a perturbation of the nervous system, which produces functional insufficiency of the heart, paralytic dilatation of the dependencies of the portal system, the intestinal secretion, and the ischemy, or general arrest, of the circulation.

"*It appears, therefore, to result from our experiments that the nervous theory of cholera is that which best explains its pathological physiology.*"

The publication of this conclusion, which was arrived at by Messrs. Sicard, Poucel, and Taxis, who formed part of the Commission nominated by the Société Nationale de Médecine* of Marseilles to conduct a series of inquiries concerning cholera, and

* The discussions and very important results of the researches conducted by commissions of this Society, which were published in the successive numbers of the *Marseille Médical*, referred to in previous pages of this essay, have been collected and issued by the Society as a separate report, entitled "Choléra de 1884: Discussions et Recherches réunies et publiées par les soins du Dr. Ch. Livon, Sécrétaire-Général." Marseilles, 1885.

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which was read to that Society on November 13, 1884, is, I believe, the first decisive and authoritative recognition of the doctrine which I have long advocated—viz., that cholera is a disease of the nervous system. This recognition is, in my opinion, significant that great progress in our knowledge of the essential nature of cholera is being made; and to me, personally, it is, of course, peculiarly gratifying; but those of my readers who have duly apprehended the pathology of cholera expounded in Section III. of this essay will not need to be reminded that I am very far from concurring in the doctrine that all the anatomo-pathological lesions of cholera are "solely dependent on troubles of cardio-vascular innervation," and that these induce "functional insufficiency of the heart, paralytic dilatation of the dependencies of the portal system, and the intestinal secretion." According to the doctrine explained in that section, whatever "functional insufficiency" of the heart may exist is not due to enfeeblement of the nervous ganglia in the substance of the heart, except in so far as their blood-supply may be insufficiently oxygenated, but is due to mechanical distension of the right heart produced by obstruction of the passage of the blood through the lungs. This obstruction itself is, indeed, as I have explained, caused by morbidly excessive contractions of the pulmonary blood-vessels and of the bronchial tubes—contractions which, in turn, are due to excessive stimulation of their muscular coats by the tumultuously excited sympathetic ganglia functionally related to them. As the blood is being slowly but continuously poured from all parts of the body into the right heart, and as, owing to the pulmonary obstruction, the outgoing of the blood does not equal its incoming, mechanical distension, and, therefore, difficulty of the right heart to contract on its contents are inevitable. This alleged "functional insufficiency" of the heart is, as I maintain, exactly of the same nature as that which I have proved to obtain when an animal is put to death by causing it to inhale chloroform. In this case the passage of the blood through the lungs, owing to the absence of the oxygen contained in the air which is displaced by the chloroform vapour, is impeded, the branches of the pulmonary artery, the right auricle and the right ventricle are greatly distended, and the movement of the heart comes to a stand—not because the heart has lost its normal force, but because the resistance offered by the pulmonary capillaries to the passage of the non-oxygenated blood through them more than counterbalances that force. If the chest of an animal just killed by the inhalation of chloroform be immediately opened, the heart and pulmonary artery will be found to be distended as stated; and if now the pulmonary artery be divided, thus relieving the distension of the right heart, it will begin to beat

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again, and may continue to beat, as I have seen it beat, during ten or twenty minutes afterwards. The condition of the right heart and pulmonary artery of patients in the *algide* stage of cholera is, I affirm, like to that of the right heart and pulmonary artery of an animal chloroformed to death, and hence if such patients, even when quite pulseless, are made to breathe oxygen, great temporary relief is experienced, the distressing anxiety which they feel (and which, as I believe, is mainly due to the condition of the heart just described), ceases almost wholly to be felt, the cyanosis lessens, and the pulse returns. Giving oxygen to a choleraic patient in the *algide* stage is, so far as the state of the heart is concerned, the equivalent of dividing the pulmonary artery of an animal after it has been put to death by causing it to inhale chloroform: dividing that artery enables the heart of the animal to beat again; and giving oxygen, by inhalation, to a man in the algide stage of cholera makes the pulse return, or, in other words, increases the action of the heart. Both experiments alike prove that the heart had not lost its power, but owing to pulmonary obstruction was unable to manifest it.

Moreover, I find myself wholly unable to admit the existence in cases of *algide* cholera of "*paralytic* dilatation of the dependencies of the portal system;" or to ascribe the phenomenon designated by this phrase, as well as "the intestinal secretion" characteristic of that disease to "cardio-vascular innervation." But as I have already adverted to these questions, I need not dwell upon them here.

The evidence which I have now adduced in support of the proposition that cholera is not contagious might be indefinitely increased, but it amply suffices, as it seems to me, to prove that contagiousness is in no sense of the term a characteristic of cholera, and, in so far as it does so, to confirm implicitly the truth of the hypothesis above propounded. Facts, however, of another kind—viz., those illustrative of the mode of onset of the disease—point indubitably to the same conclusion.

The literature of cholera teems with evidence that when the disease appears in cities it very generally attacks individuals in different localities, who have had no communication with each other, either simultaneously or in very quick succession. As a general rule it gives warning of its approach in the shape of premonitory diarrhœa; but the suddenness and simultaneousness of its deadly onslaughts at different points far distant from each other are only too often most impressively manifested. When it appeared in Lord Hastings' camp in 1817, it destroyed 5,000 people within the first five days of its visitation. It broke out in

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Paris in 1832 on the same day, and, so to speak, at the same hour, in four different quarters; within eighteen days from its first appearance it had reached its climax, had invaded every quarter of the city, and had already destroyed 7,000 of the inhabitants. Lieut.-Colonel Ross, who was at Kurrâchee in 1846, has informed me that "early in the morning of the 14th of June of that year, when there was no communication by sea with Bombay (the harbour being inaccessible to the native boats or steamers in consequence of the stormy state of the sea during the monsoon), and when neither in the native town of Kurrâchee (containing about 25,000 inhabitants) nor in any other native town or village of the district was there any case of cholera signalized, the Sergeant-Major of the 86th Royal County Down Regiment was seized with 'Asiatic' cholera, and died two or three hours afterwards;" and that "upwards of forty men were destroyed by it the following night." According to Mr. Thom's report of that terrible visitation, the disease suddenly burst forth in a few hours in every European regiment, whether in camp or in barracks, in every tent and in every house, and it was at its acme in forty-eight hours afterwards, when, instead of spreading further, it gradually and steadily declined. Dr. Wakefield, in his book on "Asiatic Cholera," says that, when in India, he, on several occasions, saw cholera present itself simultaneously in different parts of the same village, but exclusively along the course of a straight line passing through the locality. He also records that, about 1869, a dust storm, accompanied by rain and hail, burst over the fortress of Gwalior, which is 300 feet high, and which until then was free from cholera. The storm had scarcely ceased when the disease suddenly broke out at several different points of the fortress, and at places surrounding it on the rock. Lieut.-Colonel Ross, who in 1862–3 had magisterial charge of the bazaar and native town of Neemuch, in Rajpootana, states that on one occasion of a sudden fall of rain, accompanied by small eyeless fish (!), two or three weeks before the monsoon was due, and when no case of cholera had been signalized in any part of the town or surrounding districts, cholera broke out, attacked the 2nd Bombay Light Infantry and the Royal Battery of Field Artillery in garrison there. Men sick in hospital who had not been out for some time, or in any way exposed to a possible contagious influence were seized; and the patients in one line of beds became victims of the disease, while those in the beds forming an opposite line escaped. The actual outbreak of the epidemic which prevailed at Spezia in 1884 is alleged to have occurred immediately after a storm, and within a few hours afterwards to have destroyed the people by forties and fifties in different parts of the town.

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The same distinctive feature of cholera—viz., its outburst at the same time in different places remote from each other—was observed both in France and England during the epidemics of 1849, 1853–54, and 1865. Physicians in the United States report the same phenomenon, which was especially noticeable at Memphis on the Mississippi as well as at New Orleans in 1848 and at Philadelphia in 1849. Notwithstanding the vast array of facts here adverted to—facts which prove absolutely that cholera may originate afresh and independently in any locality presenting conditions conducive to its development—believers in the existence of a specific poison as the cause of the disease persist in maintaining that that poison is generated in India, and that, directly or indirectly, it is imported from thence and diffused from time to time over different parts of the globe! In 1883 the assertion was made, and was echoed and re-echoed by the Continental newspapers, that the cholera epidemic which devastated Egypt was brought there by the steamer *Timor* from Bombay; and yet it was proved that there was no case of cholera on board, either during the passage or when the steamer reached Port Said, and that cholera had existed in Egypt some months previous to the outbreak at Dametta on the 22nd of June. Again, it was alleged that in 1884 cholera was imported into Toulon; but the first result obtained and proclaimed by the commission of inquiry instituted by the Government was a conviction of the physical impossibility of assigning the origin of the outbreak of the epidemic to an importation of the disease, or of the germs of it, from without. Within the first twenty-four hours of the appearance of cholera at Toulon, there were five fatal cases, which occurred in different parts of the town distant from each other, the patients having had no communication with each other. On the 22nd there were thirteen deaths, says Dr. Bourgarel, "at the most opposite points and everywhere at the same time." In like manner "the first fifteen deaths from cholera in 1884 at Marseilles took place, without exception, at fifteen different points, in fifteen different streets, and more or less distant the one from the other." In short, the more the history of the origin of cholera epidemics is studied the clearer and more assured becomes the conviction that the disease is not spread by means of contagion.

Moreover, epidemics of truly contagious diseases do not cease suddenly; but cholera often does so—just as it appears, so it sometimes disappears, suddenly after a storm. In India this fact has often been observed. Cholera has been known to cease there after a heavy fall of rain. It declined after a hurricane which took place in Madras in 1818. A regiment suffering severely from cholera in camp, on the march has been known to lose it when getting into

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barracks. Prof. Goodeve mentions that "H.M.'s 63rd Regiment suffered extremely during the greater part of its march from Poonah to Bellary, but entirely lost the disease in two or three days after getting into barracks in the unhealthy station of Bellary."

"There are three localities," says Dr. Poucel, "which follow each other, and which, equally, are placed along the Sorgne and the railway from Cavaillon to Avignon: these are Gadagne, Le Thor, and L'Isle-sur-Sorgne. Cholera ravaged Gadagne and L'Isle-sur-Sorgne, but left Le Thor untouched. Nevertheless, Le Thor is between the other two localities, and takes its water from the Sorgne, which had, of course, traversed one of the contaminated districts."

It has been alleged erroneously, and the *Times* repeats the error, that the epidemic of cholera at Marseilles in 1884 was conveyed thither from Toulon; but, as a matter of fact, the first case that year at Toulon was that of a sailor who died June 14, whereas on the 7th of the same month a man employed at the gas-works, named Jean Pronzati, died of cholera at Marseilles. Five or six days afterwards, a workman in a sugar refinery died, his symptoms being thoroughly characteristic of cholera. A third case, that of a man who had had diarrhœa for three months, also proved fatal. These three cases were described by M. Giraud, of Marseilles, who said that "each year in his quarter he sees some isolated cases of 'sporadic cholera,' but the intensity and rapid succession of these three cases in the same quarter had struck him." M. Jubiot added that "these three cases undoubtedly preceded the epidemic at Toulon, which did not declare itself until June 13." These three cases remained isolated, no others occurring in the neighbourhood of them. Of course, therefore, they were pronounced cases of "cholera nostras." *

During the conference with Dr. Koch at Marseilles, Dr. Sugké stated that in 1875 he was at Hamma, a town in Syria, situated on the banks of the Orontes, in the middle of a desert, and that nevertheless cholera broke out there, and in a short time destroyed from 300 to 400 persons. Dr. Koch having asked if the epidemic really broke out thus suddenly, Dr. Sugké replied affirmatively, and added that it was "absolutely localized." Still Dr. Koch was of opinion that it originated in India!

Dr. Poucel mentioned the remarkable fact that in South America, where cholera had never been known before, it broke out suddenly with great violence. A battle took place at Humaïta, at the south extremity of Paraguay, at the confluence of

* *Marseille Médical*, Aug. 30, 1884, p. 493.

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the Parana and Paraguay: 600 bodies of men and horses remained on the ground putrefying. Two days after the battle, cholera declared itself in both camps, and afterwards declared itself at Santa-Fé and at Buenos Ayres, where it made many victims. Credulity needs to be very strong to develop the belief that in this case the epidemic was originated by cholera germs conveyed from India. The following fact is significant:—One of the sisters of a Catholic institution in the Rue Sainte-Victoire, Marseilles, died of cholera six hours after she was first attacked. During more than twenty days previously she had seen no person from without, and during fourteen days after her death no new case declared itself in the house, which has not less than 500 inmates.

Again, it is well known that individuals who, while suffering from cholera, are removed to healthy situations, do not become centres of a new infection. Instances are adduced in opposition to this statement; but their force is lost in the overwhelming number of those which may be advanced in support of it.

"There is no exaggeration in the statement that at least 30,000* inhabitants of Toulon and Marseilles fled from those two theatres of the epidemic; this large number of men and women were, according to the believers in the contagiousness and importability of cholera, suffused with the choleraic principle, and ought to have carried it with them, and to have sown the germs of the disease as they went along. There were thus 30,000 experiments, the results of which we can now appreciate. Well, what has come of the dissemination of the cholera poison in every locality overrun, and now or recently inhabited by these fugitives? In fact, nothing: in no town, in no village, I do not say between Toulon, Marseilles and Paris, but in all the countries of Europe, has there been one new cholera focus, the origination of which can be attributed to a traveller from Toulon or Marseilles."

This important statement made to the Academy of Medicine by the eminent authority, Dr. Jules Guérin, is confirmed by Dr. Rochard, who, in his despatch of June 27, 1884, to the Minister of Marine, said:—"Up to the present time there is not a single case in the hospitals, not a single fact of transmission either in the surroundings of the patients, or in the town, or in the adjoining villages, where some of our cholera patients have gone to die."

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* According to the most recent and trustworthy inquiry, this number is much below the truth: "it has been generally estimated that from 60,000 to 80,000 inhabitants left Marseilles and its suburbs. The emigrants belonged to all classes of society; the working classes furnished a very large contingent." —*Rapport sur l'Epidémie de Choléra qui a régné en* 1884 *dans le Département des Bouches-du-Rhône*, p. 34. Marseilles, 1885.

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SECTION VI.

The Prevention and Avoidance of Cholera.

The attainment of correct conceptions concerning the nature and causes of cholera is one thing, but to be duly armed in readiness to meet its attacks, and to know how, effectually, to resist them, is quite another thing. The truth of the adage, "knowledge is power," may, however, be exemplified, I believe, in respect to cholera, as it is in all other spheres of human experience. Having learnt what are the several factors in the production of cholera, and what are their several powers, positive and comparative, when acting separately or conjointly, we have learnt that some of them are avoidable altogether, some of them partially avoidable, and that though the cosmical factors are not avoidable, their power for evil is reducible to a minimum by preventing any co-operation with them of one or more of those factors which are capable of being subjected to our control.

In a very large number of cases, cholera is induced by the conjoint operation of a cosmical cause, and of one or more of those which are avoidable, and would not be developed without their conjoint agency. When adverting to infantile diarrhœa, I showed that, as a rule, the disease is the product of at least three factors—viz., (1) extremely rapid growth, and, therefore, extreme vascularity of the nervous system; (2) irritation of the dental nerves; and (3) great solar heat. Youths, after the advent of puberty, may cut their wisdom-teeth, and persons of any age may suffer extremely from dental disorders causing great constitutional disturbance, in the midst of an especially hot summer, but one of the three factors just mentioned—namely, extremely rapid growth of the nervous system—being absent, the other two are insufficient to produce fatal diarrhœa, which the three conjointly engender to an appalling extent. Or, again, in the case of the child, if either of the other two factors, dental irritation or summer heat, be absent, the two remaining are generally insufficient to engender the disease; but of course they would suffice if the action of one or both were especially energetic. In like manner great summer heat, insufficient, however, to produce cholera in inhabitants of lofty positions, may easily produce it in persons dwelling in low-lying regions; but abstract the factor, heat, and such persons may continue to occupy low sites with impunity. Prolonged marches, pilgrimages, and ordinary travelling on foot in temperate climates, do not induce cholera, because each of them is a feeble factor, and needs therefore the conjoint action of an especially powerful one in order to become

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effective: the summer heat of Europe is not sufficiently powerful, but that of India is, and hence the reason why these factors do not manifest themselves as causes of cholera in Europe, and why in India they do. Noxious effluvia, impure water, bad food, eating to excess, purgative medicines, opium, alcoholic drinks, and, above all, fear, are severally able, in combination with one or the other of the cosmical or epidemic forces, to engender cholera in temperate as well as in tropical climates; and it is obvious that if two or more of these endemic, or more or less avoidable factors, co-operate with a cosmical one, the cholera engendered will, in proportion to the number of factors which are co-operative, be the more rapidly developed, will be of a type increasingly severe, will be more fatal, and, in cases of recovery, will be recovered from with more difficulty.

The lesson inculcated by these considerations is easily learnt: in tropical climates, he who is intent on escaping cholera, must avoid *all* its endemic or avoidable causes during *every part* of the year; but in temperate climates a less rigorous discipline is consistent with comparative safety. Of course in proportion as climates increase in resemblance to those of the tropics, the statement just made respecting tropical climates becomes applicable to them. During winter in Central and Northern Europe, its inhabitants may inhale noxious effluvia, may drink water containing a considerable amount of organic matter, may eat bad food, may eat good food to excess, may take excessive quantities of alcoholic drinks, may even take opium or purgatives as freely as they please without inducing cholera; but during ordinary summers, to do all or any of these things is attended with danger of developing the so-called "sporadic" or "European" cholera, and during unusually hot summers, especially if they are characterized by the presence of an "epidemic influence," is very likely to bring on what is called "Asiatic" cholera.

It is evident that some of the avoidable causes of cholera just mentioned are easily avoidable at any time; that some are more or less easily withstood, and that even gluttony and drunkenness, together with the several phases of indulgence in alcoholic drinks between drunkenness and temperance, may be to a considerable extent restrained. We know also that the evil effects of bad water may be to a great degree, if not wholly, neutralized by boiling and filtering it. A good deal also can no doubt be done by the inhabitants of even the least cleanly and healthy dwellings to free them from their impure air and sickly odours, and generally to render them healthier than they usually are.

I confess, however, that I have not much hope that the measures tending to avert attacks of cholera just indicated, are likely to be adopted to any notable extent by the present

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generation. The habit of drinking to excess, if once thoroughly established, is eradicable with great difficulty; and even the temporary arrest of that habit in the presence of danger can scarcely be expected from those classes of the community most prone to indulge in taking alcoholic beverages. Though the danger from drinking water containing organic matter may be minimized by boiling and filtering, it is probable that the cost of fuel for boiling it and of an effective filter would prevent the great majority of lower-class families from either boiling or filtering the water before they drink it; and their unwillingness to incur such an expense is likely to be strengthened by the consideration that cholera is but an occasional and comparatively rare visitor. Those inhabitants of inferior and more or less unhealthy neighbourhoods, who are acquainted with the causes of the unsanitary conditions which prevail, and who would be glad to co-operate in removing those causes, feel painfully that the work to be done exceeds their powers, and that unless the owners of the dwelling-houses in the locality will effect the needful improvements, those conditions must continue to be endured, at least until some governing power, municipal or otherwise, is constrained to intervene. In short, it seems to me that the general abolition of the abolishable causes of cholera will be one of the many general results of human improvement manifesting themselves simultaneously in numerous and different directions—physical, intellectual, and moral—as consequences of general and efficient education. Until then, those cosmical forces which determine the advent of cholera at different periods will continue pre-eminently fatal to the generally intemperate, and especially to the intemperate drinkers of alcoholic liquors, to the drinkers of water containing a large amount of organic matter, and to the dwellers in localities unhealthy in consequence of defective drainage and other deleterious conditions already alluded to.

If cholera visits any place several times at more or less distant intervals, such repeated visits should, in my opinion, be regarded as indubitable evidence that that place is in urgent need of sanitary reform of some kind. Persons residing in it, or in the part of it in which cholera has declared itself, are partaking of its insalutary influences, and are therefore predisposing themselves to be easily acted on by one or other of the cosmical forces just mentioned. Now, while repudiating the doctrine that cholera is contagious, and consequently having no fear that the disease may be communicated by the sick to the healthy, I am strongly of opinion that persons ought not needlessly to expose themselves to such insalutary influences, especially when they are more than ordinarily dangerous; and therefore, that when cholera appears in such a place, every person not needed to tend the sick, and

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not constrained by other causes to remain, should leave it as speedily as possible. Of course, such persons can safely return, at least so far as cholera is concerned, as soon as the epidemic influence has completely ceased to operate.

It has been shown that mere lowness of site is itself conducive to the development of cholera, and hence the well-known fact that the disease is specially wont to manifest itself in towns or villages along the borders of rivers. When, by means of its herald, "summer diarrhœa," cholera threatens to invade such low positions, safety should be sought by moving to higher levels. Many of my readers will probably exclaim, "But to carry out these prescriptions will, as a rule, be extremely difficult, and in a large proportion of cases impossible!" I know it, but still can only rejoin—the inhabitants of such places who remain in them when cholera is hovering near them do so at the peril of their lives.

Having, in the foregoing remarks, indicated the lines of duty in respect to the removable or avoidable causes of cholera, I will now advert to certain causes wholly beyond human control, and not wholly within the reach of human comprehension. I refer to what I have designated the cosmical causes of cholera.

I have already said that there are reasons for believing that cholera may be originated by one factor alone. It is indeed probable that in certain cases heat and electricity are each sometimes solely operative in producing the disease, just as heat alone produces the disease called "sunstroke." Such cases are often what the French call *foudroyants*, and are known in India as *cholera-sicca*—cases characterized by the absence of premonitory diarrhœa, and by the awful swiftness with which men seemingly in the full vigour of health are destroyed. "Thus at Kurrâchee, in 1846, people are said to have died within less than one hour from the time they were seized." Many died within a few hours from the time of seizure, and in these cases, says Mr. Thom,

"Vomiting and purging were not always present. Sudden collapse, ending in profuse sweating, were the most prominent symptoms—in fact, asphyxia had already taken place. It was often found that the pulse had ceased at the wrist, the eyes turned up, the voice hollow and feeble, before the natural hue had given way to that horrible lividity which is characteristic of the disease, so instantaneously was the power of life arrested."

Dr. Milroy states that at Teheran, in 1846, "those who were attacked dropped down suddenly in a state of lethargy, and died at the end of two or three hours without convulsions or vomiting, but from a complete stagnation of blood."* Although the

* It is worthy of remark that these two examples of extremely sudden attacks of cholera occurred in the same year—one in India and one in Persia; whether or not they both occurred at the same time of the year is a question of great interest, which, however, I am not now able to answer.

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majority of cases at Kurrâchee in 1846 lasted longer than those just mentioned, yet the swiftness of onset, development and decline of the disease presented the likeness of a storm suddenly bursting over the city. I have already said that it seems to me not unlikely that when the mode of attack of cholera is gradual, is characterized by premonitory diarrhœa, and presents itself over a wide area, the main factor in its production is solar heat; and that when the mode of attack is sudden, concentrated and intense, the main factor in its production is that form of solar force we call electricity. If this opinion be correct, it is probable that the main force operative in producing the two epidemics just mentioned, and others with like characteristics, was electric. At Kurrâchee the soldiers were in ordinary camp tents—fourteen men in each. The tents of the 1st Bombay Fusileers were arranged in five companies—forming lines on each side of a wide space which divided the two wings of the camp. The men of the first line on one side were all carried off; whereas, of those in the first line on the opposite side, all escaped! I have already mentioned, on the authority of Lieut.-Colonel Ross, on whose authority I also give the fact just named, that the patients in one line of beds in hospital at Neemuch "became victims of the disease, while those in the beds forming an opposite line escaped." I may also repeat here the statement of Dr. Wakefield, already quoted, that, when in India, he, on several occasions, saw cholera present itself exclusively along the course of a straight line, passing through the locality in which the attack occurred. The nature of these attacks is only intelligible on the hypothesis that they were electric. Now the question arises, whether in those cases in which cholera is seemingly due, either exclusively or mainly, to cosmical forces, anything can be done, independently of medical aid, to avert its attacks.

SECTION VII.

Disinfection and Quarantine.

The logical conclusion to be drawn from the whole argument embodied in the preceding pages is that quarantine is both useless, and worse than useless. If cholera is neither contagious nor infectious, healthy persons cannot be protected against its attacks by being prevented from coming in contact with choleraic patients.

Persons affected with sunstroke or epilepsy do not exhale or

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emit through any channel germs of either of those diseases capable of generating them in other persons; and I venture to affirm that the foregoing facts and arguments demonstrate that the neuro-sthenia productive of the group of phenomena we call cholera is no more contagious or infectious than are those diseases, and that neither the clothes nor the dejecta of cholera patients are capable of communicating the disease to other persons. How then can disinfection or quarantine avail as a protection against cholera? What can be the use of the so-called "disinfection" of persons and things not previously infected? These questions can only be truly answered in the negative: no single fact can be adduced proving that such disinfection has ever prevented the spread of cholera. In 1884, owing chiefly to the inspiration of the French Academy of Medicine, "disinfection" was rigorously practised on the continent of Europe: passengers crossing the frontiers from one country to another at many railway stations were remorselessly fumigated, all their several articles of personal luggage were fumigated, and even letters *in transitu* were stopped and fumigated! And now the whole of this vexatious and costly procedure—costly alike both in money and time—is admitted, even by those who advocated it, to have failed signally to achieve the object which it was expected to accomplish—to have been, in fact, utterly useless except to effect loss and suffering. Happily, in this case, the lesson taught by bitter experience has not been taught wholly in vain: the sub-committee of the Sanitary Conference at Rome has declared "that land quarantine and sanitary cordons are useless," and that "disinfection of merchandise and postal packets is superfluous." These concessions will surely make the public thankful for small mercies. On the other hand, the "Technical Committee" passed, among other regulations to prevent the spread of cholera by land, the following:— .

"International express trains must be changed in passing from an infected into a non-infected country; a surgeon must accompany every such train, to take the necessary steps in the event of any one falling ill with the disease; and every large station must have an isolated room for the reception of such cases."

This regulation was passed in face of the demonstration by Dr. T. R. Lewis of its uselessness: he showed that the construction of the great trunk railways in India has neither changed the lines along which cholera manifests itself nor hastened the rapidity of its manifestations. Several other equally useless regulations involving the enforcement of quarantine were also passed, and then the Conference was finally adjourned. I presume the regulations already passed will have no binding force unless the Conference meets again to complete its labours; and,

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as remarked by a writer in the *Medical Times*,* "it is becoming very evident that no practical result is likely to flow from it." Indeed, it is very difficult to imagine how any useful practical result could flow from it.

A number of first-class physicians who have passed their lives in different countries, whose experiences, therefore, have been very diverse, who have entered on the study of cholera with widely different preconceived, or theoretical, views concerning it, may contribute greatly by communicating authentic facts relating to it, and by exhaustive discussions of it in its manifold aspects, to enlarge and correct their ideas concerning it, and thus prepare the way for the ultimate attainment of a thoroughly verifiable, and therefore truly *scientific*, knowledge of the nature of the disease. It is such knowledge, and *only such knowledge*, that can constitute a really trustworthy and permanent basis for a code of therapeutical and prophylactic regulations capable at once of combating cholera successfully when it presents itself, and of preventing its development. But how can any rational action for the attainment of these objects issue from a Conference of such heterogeneous and chaotic elements as those which constituted the International Sanitary Conference at Rome in June, 1885? For example, one member of the Conference, representing Germany (Dr. Koch), affirms that a single comma-bacillus, gaining access to the alimentary canal of a man, is sufficient to kill him; another member, representing England, affirms that inasmuch as various microscopic organisms are found in cholera dejecta, the selection of the comma-shaped bacilli as the *materies morbi* of cholera appears to be entirely arbitrary, and that comma-shaped bacilli are ordinarily present in the mouths of healthy persons. Dr. Koch maintains that desiccation most surely kills the comma ("cholera") bacillus, and, therefore, that it is expedient to cease watering the streets, and to prevent water from running along the street-gutters; whereas a large majority of the members of the Congress cherish prejudices in favour of cleanliness effected by systematic washing of streets, persons, and articles of domestic and personal use as a powerful prophylactic against cholera. Drs. Brouardel and Prout, representing France, insist, as many other members of the Congress do, that cholera always originates in India and that it is conveyed thence by means of ships to Europe, although no reply was given by any member of the Congress to Professor Lewis's inquiry whether any delegate knew of a single instance of cholera having been imported into Europe by an English ship; the non-contagionists, among whom were the English re-

* June 13, 1885.

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presentatives, Professor Lewis and Sir William Guyer Hunter, affirm, if I am not mistaken, that cholera is capable of originating *de novo* in any locality where suitable conditions for its generation coexist, and that it is not brought from India to Europe, but that its foci of independent origin are probably as numerous in Europe and America as are the places in which it appears.

Indeed, the grounds of difference from each other on questions both of principle and detail which the several members of the Congress exhibited are so numerous, that a substantial agreement between the delegates either respecting the nature of the quarantine regulations to be adopted, or whether even quarantine should be continued at all, is hopelessly impossible. Considering the various and fundamentally divergent views of the delegates, an impartial spectator would probably conclude that the several Governments who appointed them had consciously assisted in the perpetration of a grim joke, or solemn farce on a grand scale, for the astonishment, at all events, if not for the edification, of Europe, and, perhaps, also, to excite its contempt for the so-called "medical science" of the last quarter of the nineteenth century. However this may be, I cannot help thinking that all sensible persons will be of opinion that until the leading physicians of the world can come to an agreement concerning the essential nature of cholera, concerning its causes and their mode of operation, and, therefore, concerning the measures likely to be most conducive to its prevention and cure, they had better restrict their discussions of it within purely academical limits instead of striving to incorporate their crude, or unverified, theories in practical forms which, after shamefully impeding international intercourse and exerting a disastrous influence on the world's commerce, are at length felt to be as intolerable as they are absurd, and, therefore, are, one after another, annulled.

Just as sanitary cordons, other kinds of land quarantine, disinfection of railway passengers and their luggage, of merchandise, as well as of postal packets, which in 1883 were eagerly insisted on by the French leaders of the crusade against cholera, have since been all alike declared useless and, theoretically at least, have been abandoned, so, at no distant time, the custom of subjecting vessels to quarantine on account of cholera will, I venture to prophesy, be authoritatively declared not only useless but fraught with very serious injury to all persons concerned in it except the officials who effect its execution, and will, therefore, be wholly and everywhere abrogated.

There is a maxim said to be acted on in the law-courts, to the effect that a barrister representing a bad cause best serves his client by doing all he can to blacken the character of his opponent's witnesses. Tactics of this kind seem to have been

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adopted in respect to the opponents of quarantine. According to the Berlin correspondent of the *Times* of June 8, 1885, "the opposition of England to the quarantine regulations proposed by the Sanitary Conference for the Suez Canal is put down by the German journals solely to selfish commercial considerations." How thoroughly the belief prevails on the Continent that this sort of sordid motive moulds the opinions and shapes the endeavours of Englishmen in the matter of quarantine, is strikingly manifested by the following edifying paragraphs, which I translate literally from the *Marseille Médical* of October 30, 1884:—

"The English, who are partly protected by their climate from the influence of cholera—the English, the aristocracy of which forms a class apart, and which knows well that by means of hygiène and comfort (when they are to be had) it is possible to ensure safety from cholera, as well as from intermittent fever, would consent *without compunction* [*sans douleur*] that the whole universe should enjoy the benefits of endemic cholera, provided that every obstacle to the transport of their products be removed.

"This thought, raised to the height of an economic system, induced them to license the Sanitary Council of Alexandria in 1883, and induced them to declare that the cholera in Egypt during that year, which destroyed not less than 50,000 victims, was a local epidemic of no importance.

"It is the same thought which has induced them to promote the spread and adoption of the belief that cholera has now disappeared from Calcutta, Madras, Pondichèry, Bombay, &c., in short, from all the great ports the exports from which concern their industry, and which, from the beginning of last year, has made them carry on in Europe a campaign against quarantine. . . . .

"It is now demonstrated that, according to the English, cholera ceases to exist from the moment that the aristocracy and the great manufacturers do not die of it!"

This astounding revelation was occasioned by the fact that Dr. Koch, in the course of a conference with the Marseilles doctors, told them that an aqueduct has been constructed for the conveyance of water from the Hoogley to Calcutta; that this water is taken from the river several miles above the city; that before being distributed it is well filtered; that the aqueduct has supplied Calcutta with water since 1870; that between 1865 and 1870, notwithstanding the fact that great improvements had been made in the drainage of the city, no appreciable diminution of cholera was observable, but that as soon as the aqueduct came into use cholera lessened; and that since 1870 there has not been above a third of the number of cases which were wont to occur before that date. Dr. Koch also said:—

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"Formerly, cholera appeared frequently and in a severe form at Pondichèry. Within the last few years artesian wells have been sunk there, and since then cholera had disappeared. In 1882 a rumour was suddenly spread abroad that Pondichèry had lost this beneficent immunity. I questioned Dr. Furnell, of Madras, as to the truth of this rumour. He had frequent relations with Pondichèry, and was thoroughly acquainted with the facts of the matter. His opinion was that a few cases had indeed occurred here and there in the town, but precisely in those places where there were no artesian wells."

These statements were regarded by the Cholera Commission of Marseilles as due to the fact "that Dr. Koch had been the victim of his own childlike trustfulness in placing implicit confidence in the official, or officious, assertions of the English authorities who had evidently led him into error." The reason why these wicked officials did so is fully explained in the terrible denunciation of "les Anglais" given above. Englishmen are accustomed to see this sort of thing, and therefore expect to find it, in French newspapers below the rank of the *Journal des Débats* and *Le Temps*, but to encounter it as forming an important part of a grave report of the conclusions of a commission of scientific men concerning cholera is indeed a surprise. The fact of its presence there may seem to all Englishmen, and, I hope, to many Frenchmen, incredible; nevertheless it is strictly true: the document is authenticated by the signatures of the body of scientists who hold up the mirror in which the English may contemplate at leisure the real nature of their moral lineaments. These are the signataries: "*Dr. Adrien Sicard, A. Taxis, Dr. Bouisson, Dr. Queirel, Dr. Poucel, J. Chareyre, Dr. Livon.*"

Now, in presence of the fearful condemnation pronounced by this formidable array of inquisitors sitting in solemn judgment, against any one who dares to lift up his voice against the practice of quarantine, he who demonstrates that it is at once useless and baneful runs the risk, it seems, of being denounced as one of those demons called Englishmen, who, so long as they themselves, being safe from cholera, can live in comfort, and can be assured that no obstacles will impede their commercial operations, contemplate calmly and without suffering the universal prevalence of the disease in its endemic form.

I am obliged to confess that I am an Englishman, but I do solemnly declare that I am not an English aristocrat—not even an English manufacturer, and that I have no share whatever in any English commercial enterprise. Indeed, I do not even live in England. These facts will, I hope, be considered as an adequate justification of my plea that my misfortune of having been born in England may not be remembered against me by

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my French, or German, judges, thus preventing them from judging righteously of my arguments against quarantine. I also hope that they will duly consider that, in reality, it is not I that speak on this question, but the large body of facts which are presented in the foregoing pages, and which speak for themselves—declaring in one loud accord that cholera is not contagious, is not infectious, and, therefore, that its development and spread cannot be prevented by quarantine. Moreover, they also declare that, *so far from imprisoning persons in the localities in which cholera may break out, Governments, seeing how cholera generally restricts its ravages to special localities, ought to encourage, by every means in their power, the inhabitants of such localities to remove themselves as speedily as possible to places, and especially elevated places, which remain free from the disease until it has disappeared.*

One very important duty in times when the diurnal range of temperature is great, as it is especially apt to be in September and October, is to take especial care that the body is kept thoroughly warm during the night. I believe many attacks would, by the observance of this simple precaution, be prevented. Then, in proportion as the disease seems to be due to solar heat, the utmost efforts should be made to ensure very free ventilation of every dwelling, to make constant use of cooling appliances of every available kind, including, especially, the daily use of cold baths, and, as far as possible, to migrate during the time of danger from low-lying to elevated ground, it being borne in mind that the temperature becomes lower and the air purer in proportion as the elevation increases. When, owing to the extreme suddenness and intensity of an outbreak of cholera, there is reason to believe its main cause to be rather electric than thermal, the affected area is likely to be comparatively limited and fairly defined. If so it will not be difficult for a large proportion of its inhabitants to protect themselves by leaving it for a time, and it is obvious that the more quickly they do so the more certain their safety. But during all outbreaks of cholera, however effective may be the protection afforded by flight from the region in which the disease presents itself, the great majority of people, being poor and constrained to earn their subsistence in the locality where they live, cannot leave it, and hence must face the enemy on his own chosen ground. Here, therefore, arises the question, what aid, if any, can medicines bring to the sufferer from cholera?

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SECTION VIII.

The Medicinal Treatment of Cholera.

In attempting to form any estimate of the comparative value of different medicines used in the treatment of cholera, we must bear in mind the following facts: the mortality of the disease differs—*first*, in different years; *second*, in different seasons of the year; and, *third*, during the different periods—the outset, development, and decline—of each epidemic. This last fact is well illustrated during the outbreak at Kurrâchee in 1846. Of the

First	100 patients	admitted into	hospital	79 died.
Second	,,	,,	,,	66 ,,
Third	,,	,,	,,	50 ,,
Fourth	,,	,,	,,	40 ,,

The treatment all the time remained essentially the same. Moreover, the effects of medicines differ widely according to the stage of the disease in which they are given. During the premonitory stage and during reaction they are powerful for both good and evil; but during partial collapse they are absorbed only slightly, and during profound collapse scarcely at all. These considerations, as well as many others which I need not recapitulate, must be taken into account before an *accurate* appreciation of the statistical results of different methods of treatment can, if it ever can, be arrived at. The following returns of the Medical Council of the London College of Physicians must, therefore, be accepted as an *approximative* estimate. It is, however, the best available, and probably sufficiently near the truth for practical purposes. When the different methods of treatment were applied in the various stages—from choleraic diarrhœa to profound collapse—the *general* percentage of deaths following each plan was as follows:—

Eliminants	71·7 per cent.
Stimulants	54 ,,
Alteratives, calomel and opium	36·2 ,,
Astringents, chalk and opium	20·3 ,,

But when the different methods of treatment were tested in their application to cases of collapse only, the results were as follows:—

Calomel and opium	59·2 per cent.
Calomel (large doses)	60·9 ,,
Salines	62·9 ,,
Chalk and opium	63·2 ,,
Calomel (small doses)	73·9 ,,
Castor oil	77·6 ,,
Sulphuric acid	78·9 ,,

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It is obvious that in a very considerable proportion of the cases, from the treatment of which the first of the above statistical statements is generalized, the absorbent process was still very active; for otherwise the wide difference in the results of the different methods would not have been possible—differences ranging over 50 per cent. In the more advanced stages of the disease, or during collapse, the differences, according to the second statement, have a range of nearly 20 per cent.—a fact which indicates that absorption in a slight degree still goes on.

It will be observed that, according to the report just given, the average number of deaths in proportion to the number of cases treated is very great—viz., 45·5 per cent. of the cases varying in severity from choleraic diarrhœa to profound collapse, and 68·1 per cent. of the cases in complete collapse. Now, in my opinion, if the medicines given in the cases in question exerted any influence, it was, on the whole, an injurious one; and this opinion is strengthened by considering how especially high is the mortality following the use of eliminants and stimulants in the first group of cases, and of calomel, castor oil, and sulphuric acid in the second. It is evident that if the sufferers had been spared the use of those drugs, the proportion of fatal cases would have been less than it actually was. All experience of the treatment of cholera during the stage of collapse by means of drugs proves that they are useless. The numerous and careful experiments made at the London Hospital in 1866, in order to determine the comparative value of various drugs said to be curative of cholera, resulted in confirming the mournful conclusion arrived at during previous epidemics of the disease—viz., that no drug yet discovered exerts any appreciable power of rescuing patients from the state of choleraic collapse. This fact, like all others characteristic of cholera, is now for the first time rendered quite intelligible by the hypothesis explained above; for whereas, according to that hypothesis, the automatic nervous centres are in a condition of intense hyperæmia, physicians are acquainted with no drug which has a distinctively ascertained power of abolishing that condition. But while, as it appears, drugs can do no good, they can do much harm, and the reason why they can do so is not difficult to understand.

When cholera has reached its most distinctively characteristic stage, designated *collapse*, the power of absorption by the alimentary mucous membrane exists, if at all, to so slight a degree that, even if drugs which are given are not ejected by the stomach, most of them, at all events, are likely to remain inert so long as the state of collapse continues. If the patient should survive that condition and reach the stage of reaction, the medicines previously administered may seriously interfere with Nature's

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efforts to emerge from that stage, or from the secondary fever which, under ordinary treatment, not infrequently supervenes. Moreover, as the essential nature of cholera has not been understood, and as the medical world is still disputing concerning its nature and causes, medicines have meanwhile been given either at random or without the guidance of any principle which has obtained extensive professional recognition. It appears from the above figures that the three drugs, the use of which has been attended with the greatest mortality, are calomel, castor oil, and sulphuric acid. Considering that purgatives are proved to be especially dangerous to cholera patients *in any stage* of the disease, we easily understand why calomel and castor oil co-operate so effectively, as they are shown to do, with the disease itself in bringing it to a fatal issue. The influence of sulphuric acid appears to be even more deadly than that of castor oil. This acid is supposed to act as an astringent; for this reason it is given in cases of hæmorrhage with the intention of restraining it, and for a like reason it has been given to sufferers from cholera in the hope of lessening the excretions; but to whatever extent it may act as an astringent generally, it will to that extent act as a contractor of the capillary blood-vessels and small bronchial tubes, and therefore, if the pathology of cholera already explained be the true one, as an intensifier of the algide condition already established. From this point of view, the extremely dangerous influence of sulphuric acid on cholera patients is thoroughly intelligible.

The drugs which, in my opinion, might be given during the stage of collapse most safely and most expediently are bromide of potassium and bromide of ammonium. Bromide of potassium so affects the nervous tissue as to lessen, it is believed, its vital activity; and bromide of ammonium, while supposed to act somewhat similarly, seems also to counteract the tendency to congestion and to facilitate the capillary circulation, especially in the nervous system. At all events, it is certain that in the treatment of the whole group of convulsive or spasmodic affections, the chief proximate cause of which is hyperæmia of the nervous centres morbidly operative, these drugs are more efficacious than any other of the vast number which have been tried. The conclusion is, therefore, reasonable, if the proximate cause of cholera be what I allege it to be, that the bromides of potassium and ammonium, which should be given together, will, in some cases, exert a beneficial, possibly a curative, influence on the disease; and this conclusion derives strength from actual experience, which attests that the use of these drugs in cases of diarrhœa is attended with decided benefit. I may add that, in any case, they are, of all known drugs, precisely those which are likely

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to work the least harm after reaction sets in. I should also be disposed to give a thorough trial to the tincture of aconite in cases in which its effects could be continuously, intelligently, and carefully watched.

It cannot be too strongly impressed on the public mind that it is during the early phases of cholera—"summer diarrhœa," "premonitory diarrhœa," or "cholerine"—that medication can exert its beneficial influence most potently. So long as the skin of patients suffering from diarrhœa retains its normal warmth, both sulphuric acid and opium may be given with safety and often with signal benefit; but in cases in which the skin has already become abnormally cool, I do not venture to prescribe either of them. My chief reliance, if obliged to rely on drugs, is then on the bromides and such other medicines as, while tending to check the discharges, will neither constringe the blood-vessels nor increase, as opium would, the hyperæmia of the automatic nervous centres.

But even as means of combating the premonitory diarrhœa drugs very often fail utterly. This fact is attested by the enormous mortality due to diarrhœa, not only in those years in which cholera presents itself in its fully developed form, but every year; for during every summer infantile diarrhœa, which I affirm to be a form of cholera, is fearfully fatal. Indeed, whoever recognizes the truth of the doctrine concerning the essential nature and causation of cholera which I have explained, can scarcely be surprised by the great fatality of the several forms of the disease. It is evident that an effectual remedy for it must be one which can exert a great, and at the same time swiftly operative, sedative power over the spinal cord and the sympathetic nervous centres, without lessening the general vitality of the patient meanwhile. As already stated, no medicine yet known can do this; and even if it could, the sufferer from cholera would, probably, be unable to derive benefit from it, seeing that precisely when he most needs it his stomach most persistently rejects it.

SECTION IX.

The Neuro-Dynamic Treatment of Cholera.

If the nature of the morbid processes constituting cholera is, as I have shown, wholly dynamic, it is in the highest degree probable that the most effective remedy of the disease will be found to be dynamic also. Now, in 1863, I discovered that the circulation in the sympathetic ganglia, as well as in the

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spinal cord, and consequently their energy, can be lessened by the application of ice, and can be increased by the application of heat, along the region of the spine. Since that time, my twenty years' experience of the reality of the discovery has been abundantly confirmed by the experience of other physicians in Great Britain, Ireland, France, Germany, the United States of America, and various parts of the British Colonial Empire. In my exposition of the nature of cholera, I have divided all its symptoms, prior to the stage of reaction, into two groups, and have explained how one group is produced by morbidly excessive activity of the spinal cord, and how the other is produced by a like condition of the ganglia of the Sympathetic. Now, if this explanation be true, and if the power of lessening the energy of those nervous centres, alleged to have been discovered by me, be a reality, the possibility of treating cholera both scientifically and successfully is at length within our reach. And this presumption is greatly strengthened by experience in the treatment of many other diseases which, though not severally, but collectively, present symptoms like to those of cholera. I have proved that excessive sweating and excessive secretion by the mucous membranes—indeed secretion generally—can be restrained by a suitable application of ice along the spine; that vomiting, purging, cramps, and coldness of the surface of the body, when met with separately as symptoms of other diseases, are, in the majority of cases, capable of being subdued by the same method. I have demonstrated that the vomiting of all grades of severity incident to pregnancy, the vomiting symptomatic of various diseases, and especially the vomiting of sea-sickness, can be thus arrested. Indeed, when sea-sickness becomes very severe, it is often accompanied by diarrhœa, and assumes a remarkable likeness to mild forms of cholera; and a large amount of evidence has now been accumulated, and partly published,* proving that, in the great majority of cases, at all events, this disorder can now be remedied. A very severe form of it is presented in the case described by Dr. Lee of Philadelphia—a case which affords striking evidence that vomiting, tonic convulsions, cold sweat, and coldness of the surface of the body are remediable by the spinal ice-bag. I repeat the doctor's words:—"The effects of its application were little short of miraculous. In three minutes the retching ceased, and the spasms were calmed. In a quarter of an hour the patient had fallen into a quiet sleep, and in half an hour her hands and feet were

* See "Sea-sickness, and How to Prevent It. An Explanation of its Nature and Successful Treatment through the Agency of the Nervous System, by Means of the Spinal Ice-bag. With an Introduction on 'The General Principles of Neuro-Therapeutics.'" Second edition, 8vo.

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of natural warmth, and her face had regained its wonted colour." Now, in this case, several of the leading symptoms of cholera were present; and while the vomiting, tonic convulsions, and cold sweating were arrested by the sedative action of the ice on the spinal cord, a like action on what Claude Bernard rightly designated the "frigorific" nerve-centres, so lessened their energy as to enable the lady's hands and feet, within half an hour, to regain their natural warmth and her face its wonted colour. Surely these are the very results desiderated in the treatment of both diarrhœa and cholera.

My discovery of the power of exerting a controlling influence over the nutrition and functions of all parts of the body, and my demonstration that in man, and in all animals having a highly differentiated nervous system, diseases, whether called structural or functional, of any part, are really, as a general rule, symptoms or expressions of disorder in that system, reveal and explain the as yet seeming wonder—viz., the stopping of cramps, vomiting, and purging, and the re-generation of animal heat by the application of ice along the spine, as well as the fact now thoroughly established and apparently not less wonderful—viz., that the reactionary fever, which in a certain proportion of cases follows the collapse of cholera, is most effectively subdued by the appropriate application of heat along the same region. The more this method of treatment (not of cholera only, but of diseases generally), as well as the physiological principles on which it is based, is studied, the more thoroughly will it become recognized as truly scientific, and therefore as the dawn of a new era in pathology and therapeutics.

The general adoption of this treatment will necessarily be slow, because (1) the great body of the medical profession is still practically unacquainted with it, and only partially or vaguely acquainted with the neuro-physiology which underlies it; and (2) because people generally are horrified by a method of treatment which, in those cases in which it does not prescribe the application of heat, prescribes the application of cold, by means of ice, along some part or the whole of the spine. People will only cease to be thus horrified in proportion as they are taught so much of neuro-physiology as will enable them to understand the vaso-motor (artery contracting) functions of the great sympathetic ("frigorific") nerve, and therefore to understand the seeming paradox, but really simple truth—viz., that *precisely those persons who suffer most from bodily cold are most needing treatment by the application of ice along the spine, may be most benefited by it, and may be rendered warm meanwhile.* While this knowledge is growing, it will be chiefly those diseases for which there is no other known remedy, or none commanding

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trust, that will be treated according to the scientific principles and method here indicated: such diseases pre-eminently are epilepsy and the whole group of convulsive affections, the hitherto incontrollable vomiting often associated with pregnancy, sea-sickness, and especially cholera of all grades of intensity.

I have already published a number of cases of diarrhœa, some of them advancing into the dangerous cold stage, some of them also marked by the presence of cramps, in which the results of the treatment in question proved quite as satisfactory as were those experienced by the sea-sick lady just mentioned. Several of these cases were treated by different physicians, who have been good enough to send me reports of their experience of the method in question. Judging from the experience now had by myself and others of that method, I do not hesitate to express my conviction that if in all cases of diarrhœa, whether "infantile diarrhœa," "summer diarrhœa," or "diarrhœa premonitory of cholera," that method of treatment were promptly and properly made use of, the terrible fatality now consequent on that disorder would soon become a thing of the past.

SECTION X.

Directions for the Treatment of Diarrhœa.*

1. In every case of both diarrhœa and cholera a thorough search for every removable exciting cause of the disease should be instituted, and if any such cause be found, it should of course be removed.

2. *Simple diarrhœa,* that is, uncomplicated with any marked fall of the temperature of the body, or with vomiting or cramps, should be treated by applying the spinal ice-bag continuously until the symptoms are overcome, and for some time afterwards at increasingly longer intervals. This will generally be all that is necessary to effect a cure. Indeed, in many slight cases the application of one bag of ice completely suffices to stop the flux and to prevent its return.

3. *Severe diarrhœa,* sometimes called choleraic diarrhœa or cholerine, is in fact a mild form of cholera, and should be treated exactly as that disease is directed to be treated in the following paragraphs.

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* The "Directions" given in the text are extracted from my book of "Cases of Diarrhœa and Cholera."

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SECTION XI.

Directions for the Treatment of Cholera.

1. The successful treatment of this formidable malady by my method implies that the physician practising it possesses accurate knowledge of the principles on which it is based, as well as ability and tact to apply them correctly in each of the different cases—although presenting different physiological conditions differently combined; he must exercise the utmost possible watchfulness and untiring care; and he must resolve to insure the doing of what needs to be done both *quickly* and *thoroughly*. While ice to the spine is duly applied, its action must be especially watched, and its application must be modified in accordance with the pathological changes observable.

[The experience gained from the trials of my method of treating cholera at Southampton, impressed each observer of those trials with this conviction. When, in 1865, Dr. Wiblin strongly commended my treatment, he added—"but all your injunctions must be strictly carried out." Dr. Lake observed, "as with all other remedies of power it requires to be used with discretion, and not continued too long." Dr. Griffin, after recovering from collapse his two cases which he afterwards lost at Freemantle, says that in consequence of their distance from him the application of the ice was continued too long. Mr. Bencraft, in his letter dated October 29, 1865, says—"I have no hesitation in recording *my* conviction that *if applied in anything like reasonable time*, the ice will save every life; *but I also see that it must be carefully watched.*" Within the limits assigned to these directions, I cannot enter into the physiological and pathological reasons why ice applied along the spine of a cholera patient for a certain length of time may save life, and why if applied for a longer time it may endanger or even destroy it; I may observe here, however, that the same power which can reduce the amount of blood in the spinal cord and in the ganglia of the sympathetic from a state of hyperæmia to the normal state, may also be so used as to render those nervous centres anæmic, and that such a state is always attended with danger. It is therefore absolutely necessary to determine by careful observation of the symptoms when the ice has been applied long enough, when it needs to be re-applied, whether its application should be extended along the whole or along only a part of the spine, and if along only a part, which part.]

2. *When about to apply the spinal ice-bag, it is of the utmost importance to select one of suitable size in respect to both length and breadth.* The spinal ice-bags 24 and 26 inches long are suitable for men, those 20 and 22 inches long for women, and, of course, those still shorter and narrower for children. The widths of the bags vary with their lengths. It is therefore necessary to secure one of right length in order to secure one of right width.

3. If the lungs are healthy, the ice may, as a general rule, be applied in the first instance along the whole spine, or rather—*and this is what*

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I always mean by the phrase "the whole spine"—from the upper part of the cervical to the middle of the lumbar region, *and no lower.* As soon as the whole of the ice in the bag is quite melted, the bag should be emptied, refilled with ice, and immediately re-applied. The applications should be continuous until the symptoms abate. If the circulation in the head becomes thoroughly re-established first, the ice may then be omitted from the upper cell of the ice-bag. As soon as the vomiting ceases, and the chest and upper extremities have become warm again as a consequence of the re-establishment of their normal circulation, the ice may be restricted to the lower half of the spine until the alvine discharges cease also, and the lower extremities begin to become warm.

4. In cases in which collapse continues after both vomiting and purging have ceased, the medical attendant, guided by the special character of the symptoms in each case, must use the ice in such a manner as he judges most likely to subdue them.

[At the Hôpital St. Antoine, Paris, which I visited in 1865, there was an instructive case of this kind. An elderly woman, under the care of Dr. Buchet, had already been treated by the method he then practised, and which chiefly consisted in sponging the patient all over with water, and in then wrapping her up carefully, covering her with an abundance of blankets, and applying hot bottles to the limbs. The measures adopted wholly failed to recover the patient from collapse. She lay in a state of profound stupor: seemingly there was no cerebral action at all. The head was thrown backwards, the mouth was open; when addressed in a loud voice she gave no sign of consciousness; and she looked exactly as if dead. The head and upper extremities were as cold as those of a corpse; the lower extremities were, however, slightly warm. She was quite pulseless. There was neither vomiting nor purging, and no evidence of cramps. I applied ice to the upper half of the spine only, and altogether three bags of ice were used. The treatment began about ten o'clock in the morning, and was continued until about four o'clock in the afternoon. At this time, the temperature of the upper half of the body had increased very decidedly, the head became warm, the pulse perceptible, and the mental power had returned to such a degree that the woman answered questions intelligibly and easily. I restricted the ice to the upper half of the spine in this case for the following reasons:—Firstly, because as the woman had been extremely drained by the discharges which had occurred in the previous stages of the disease, and therefore had but little blood in her body, I deemed it expedient in view of her peculiar condition to concentrate all the spasm-relaxing effects of the ice upon the head and chest; and, secondly, because, inasmuch as there was some activity of circulation in the lower part of the body, it seemed to me that if I had placed ice along the whole spine, and had thus relaxed still further the arteries in the pelvis and lower extremities, the afflux of blood would have become still greater there, and my chances of recovering the cerebral circulation would have been less than they were by the method I actually pursued.]

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5. Allay the extreme thirst during the stage of collapse by non-stimulating fluids. It is well to give the patient the choice of several beverages, and indeed to vary them as he may feel inclined, for it is of the utmost importance to secure the co-operation of mental influences, which are no insignificant aid in allaying sickness. Good beef-tea, temptingly flavoured; arrowroot made with water, flavoured, *but only flavoured*, with brandy, and made thin, so that it may be easily drunk; *weak* tea and coffee, with but little milk while sickness still continues; lemonade, barley-water, iced-water, ice itself (to be sucked), may each be given alternately or successively with advantage.

6. If the purging should subside before the vomiting, injections of beef-tea, or of arrowroot, or of a mixture of both, should be given frequently.

7. Strychnia, opium, and all drugs which act as nervine stimulants, should be scrupulously avoided. And this remark applies to *strong* solutions of coffee and tea.

8. It is impossible to state too emphatically that *in cases of cholera under treatment by my method the life of each patient depends in great measure on the nurse in attendance.* THE REMEDIAL POWER OF THE SPINAL ICE-BAG CAN ONLY BE EXERTED WHILE IT IS RIGHTLY APPLIED. IT MUST BE KEPT EXACTLY ALONG THE CENTRE OF THE SPINE: IF IT IS NOT IT WILL DO HARM, AND HAD BETTER NOT BE USED AT ALL. The only method of keeping it in its place which does not involve incessant watching on the part of the nurse, is that of employing the "Ice-bag Jacket," described at page 33 of my volume on "Diarrhœa and Cholera;" and even then in cases of extreme restlessless slight readjustment may be frequently necessary.

[To insure the proper application of the bag; to keep each of its cells, when all are used, duly filled, and duly replenished, when needful, with ice; to keep the surface of the patient clean and dry; and duly to attend to the other and various wants of even one patient—all this is a considerable task, and one demanding for its adequate fulfilment considerable intelligence, a resolute will, and a strong sense of duty. I am painfully aware how difficult it is to secure these qualities in attendants on the sick, and how strong in the eyes of many will seem the objection to my method that its successful practice necessitates not only first-class nurses, but many more of them in proportion to the number of patients whom they attend than are usually provided in public hospitals. To this objection I can only reply: no easy method of curing cholera is yet known; drugs have been proved useless: and it behoves all whom the matter may concern, to ask themselves, "Is the life of the patient in question worth saving at the cost of providing the conditions mentioned?" If the answer is affirmative—then, as in cases of cholera, life is destroyed very swiftly, it will be well to insure those conditions not only completely, but *promptly:* an hour, half an hour, nay, a few minutes, lost in delay may seal the fate of the sufferer whose life might have been saved by *swift* as well as judicious action.]

9. The foregoing directions relate wholly to the treatment of the

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incipient and algide stages of cholera. A large number of patients whose lives are prolonged through these stages sink during the period of reaction: the fever, feverishness, or local congestions of this phase need the most studious attention, in order that they may be prevented or remedied by the judicious use of heat to the appropriate part of the spine. I have already suggested that in cases where cerebral or pulmonary reaction has become established while vomiting, purging, or coldness of the lower parts of the body persists, the ice should be omitted from the upper segments of the spine. But cases occur in which purging, at all events, as well as coldness of the lower extremities, co-exists with a full reaction, merging into slight or severe congestion of the brain, and sometimes, though less frequently, of the lungs. In these cases it may not suffice merely to omit the ice from the upper half of the spine: it may also be necessary, while still controlling the purging by retaining the ice in the lumbar and lower dorsal regions, and while, by its agency, relaxing the vasic spasms of the lower extremities, to effect some contraction of the cerebral and perhaps pulmonary blood-vessels by the application of heat along the upper third, or upper half of the spine. To do this effectually, and at the same time not to overdo it, calls for the utmost discrimination and care—discrimination with reference to the controlling force needed, and care with respect to the temperature of the water used, and the length of time during which it is applied. The changes in the circulation which may be induced by the spinal water-bag are much more rapid than those inducible by the spinal ice-bag, therefore the action of the former must be watched even more carefully than that of the latter. If heat be applied in order to prevent or subdue cerebral congestion, the forehead and pupils of the patient must be scrupulously examined every few minutes in order to ascertain whether the temperature of the former is falling, and whether the size of the latter is increasing: if so, the bag should be immediately removed, even though it may be desirable to apply it again a short time afterwards. The condition of the lungs may be ascertained not only by the feelings of the patient, by the sputa, and by the degree of ease with which he breathes, but of course much more surely by auscultation; and the use of heat must be determined by the symptoms. If there should be no local congestion but only a general feverishness, or even a decided reactionary fever, each of these conditions may be controlled by the application of heat along the whole spine. It may be stated as a general rule, that in proportion to the gentleness and slow graduation with which the fever, feverishness, or local congestion is controlled, the more satisfactory will be the result, and the less the chance of relapse. If, for example, in order to diminish cerebral hyperæmia, water of a high temperature in the spinal water-bag be applied along the upper third of the spine, the head will, in many cases, become rapidly and extremely, and it may be, in some cases of cholera, fatally cold. After such rapid and extreme contraction of the cerebral arteries, and the necessarily sudden removal of the spinal water-bag, there is danger of vigorous reaction. I advise that the temperature of the water used in these cases be the

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lowest consistent with slowly attaining the desired end. The vascular system of many patients may be influenced by water at 105° Fahr. It will, however, be found most available at 110°, and may range, particularly if the patient's body be well clothed with fat, to 115°, or even higher.

10. It must be well understood that the application of heat along the spine is capable of producing vomiting and purging after they have been thoroughly subdued by means of ice, and of causing the body again to become cold. It is impossible, therefore, to overstate the importance of the advice to use these agents only when they are absolutely needed, and to watch their effects with the utmost possible care. The intensity of the cold or heat which is applied, and the length of time it is used, must be judiciously determined and modified according to the exigencies of each case.

11. The chief things required by patients who have fairly passed through the algide and reactionary stages of cholera are good nursing and good nourishment. No medicine is absolutely necessary, unless to meet some special symptom; but I am of opinion that a gentle tonic is desirable, and that that tonic should consist of the citrate of iron and quinine. I cannot conclude these directions more appropriately than in the following words of Sir Ranald Martin, for it is impossible to over-estimate their importance:—"In cholera, in common with the last stages of violent fevers and dysenteries—as, in fact, in all cases of great exhaustion—the patient must always owe much to the horizontal position, and to careful and unremitting nursing. The most careful nursing and the most attentive watching of the patient are both of the utmost importance in this disease: and so easily is the balance of circulation fatally overturned, that a strict attention to the recumbent posture is absolutely necessary to success. In no other diseases are these simple matters of so great importance to be attended to, and in the disease under special notice I have seen many a life apparently lost from inattention to them."

SECTION XII.

Points in respect to Treatment still undetermined.

In addition to the treatment of the algide stage of cholera described in the previous section, the *simultaneous* administration of oxygen in very grave cases would, I think, probably prove useful. I have no personal experience of the treatment of cholera by means of oxygen; but, considering the facts concerning it which I am about to relate, I am of opinion that it would be especially expedient to make a thorough trial of it, (chiefly by inhalation), *in conjunction* with the treatment already mentioned, *but not otherwise.*

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At the Pharo Hospital, Marseilles, in 1884, oxygen was used. In the report of the treatment adopted at that hospital there is the following account of the treatment by oxygen:—

"The inhalation of this gas was very useful: both the dyspnœa and the feeling of pressure on the chest were, if not calmed, at least lessened in every case. We made use of no special apparatus for the inhalations of the gas. It was very pure, and was contained in india-rubber bags, from which it was inhaled by means of a tube terminating in a suitable mouthpiece. This was placed between the teeth; the mouth was carefully closed, and the patient breathed in this manner during some minutes. Inspiration was aided by gentle pressure on the bags.

"In thirty cases of complete collapse we employed oxygenated water both as a drink and as a lavement. Meanwhile, the patients inhaled oxygen. We combined these two forms of administering oxygen as follows. We had water oxygenated in the proportion of twelve volumes of gas to one of water. The patients took every hour a tablespoonful of a mixture consisting of 10 grammes of oxygenated water and 100 grammes of water. No other substance was added. Every sixth hour a lavement containing 150 grammes of water and 20 grammes of oxygenated water was given. If the lavement was ejected another was given. At the same time 20 litres of oxygen was administered by inhalation every hour.

"Under the influence of this treatment the pulse was restored, the cyanosis lessened, the anxiety almost wholly disappeared, the cardialgia was less distressing, and the vomiting ceased completely. Of the 30 patients treated exclusively by this method, 14 recovered and 16 died." *

The mortality here indicated (53·3 per cent.), which is a little above the average mortality caused by epidemic cholera, shows that very little benefit is to be hoped for from an exclusive treatment of the disease by means of oxygen.† Indeed, it is not likely that this treatment will overcome the morbidly excessive hyperæmia of the nervous centres, which I allege to be the immediate cause of cholera. But *while*, however, that morbid condition is being overcome by the continuous application of ice along the spine, the administration of oxygen promises to be a very valuable co-operative agent *for the speedy relief of the symptoms* induced by that condition. According to the experience at Marseilles, it exerts an especially great power in this respect; but, inasmuch as it does not touch the proximate cause of the malady, it deals only with effects, and the disease, continuing its march, seems

* "Rapport sur l'Épidémie de Choléra qui a regné en 1884 dans le Département des Bouches-du-Rhône," pp. 160, 161.

† As all the cases treated by oxygen are reported to have been especially grave, the mortality was probably less than it would have been if oxygen had not been administered.

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to prove as fatal, or nearly as fatal, as if oxygen had not been used at all. I am, nevertheless, hopeful that the administration of oxygen *simultaneously* with the application of ice to the spine will result in recovering choleraic patients more rapidly than they are recovered by the application of ice alone. Of course, experience alone can finally determine this important question; but at all events, I advise that, until it is determined, the inhalation of oxygen *simultaneously with the application of ice to the spine*, during the period of collapse, be thoroughly tried.

In my work, "Diarrhœa and Cholera" (p. 182 *et seq.*), I have advised the application of heat over the surface of the body generally, simultaneously with the application of ice along the spine, as well as the free use of hot drinks during the algide stage, and I have adduced what seemed to me strong reasons for these recommendations; but since the date (1866) when the second edition of that work was published, my opinion on this matter, notwithstanding the seeming force of the reasons for those recommendations, has gradually become so far modified that I now feel by no means sure that it is expedient to act upon them. After long and anxious consideration of the subject, and bearing in mind the small amount of our knowledge and experience in the matter, I believe we should act most judiciously in allowing ourselves to be guided by the instincts of our patients. Now it is certain that when in a state of collapse they crave an abundance of cold, or iced, water, and are glad to have lumps of ice in the mouth; and some physiological and pathological considerations seem to supply valid reasons why it is expedient to satisfy their longings: if the stomach be thus converted into a cold, or iced, water bag, the cold is brought into more immediate contact than it can be otherwise with the great accumulation of nervous centres, called the semi-lunar ganglion or solar plexus (which is behind the stomach), and therefore can exert a more direct influence than the spinal ice-bag can do in relaxing the arteries distributed to the abdominal and pelvic viscera. Moreover, seeing that the alimentary canal is in a state of intensely active congestion, bordering on inflammation, the temperature of the lowest part of it, and probably of the whole, being abnormally high, we have in this fact an emphatic indication that cold drinks, while supremely grateful to the patients, are likely to exert a beneficial influence. A case was communicated to the Paris Academy of Medicine, September 16, 1884, in which sucking lumps of ice, and the use of iced-water enemata, were attended with strikingly satisfactory results.

Cholera is chiefly a disease of hot climates, and when it is developed in temperate climates it is so during the months when

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the temperature is at its maximum. Now, seeing what a potent factor in the production of the disease solar heat is, I have been led to think that a part of the rational treatment of cholera may consist in placing the patient in conditions, so far as heat is concerned, as opposite as practicable to those which have induced the disease; therefore, as, during cholera epidemics, the air, which is generally very warm, is saturated with aqueous vapour, the excess of which renders breathing difficult, I have entertained the idea that by placing patients while in choleraic collapse in chambers, the atmospheric temperature of which has been reduced artificially to 60° or 50° Fahrenheit (or even lower according to the dictates of experience), we should, probably, assist powerfully in their recovery. Placing them in hot-air baths, as was done at Scutari (but without the simultaneous use of the spinal ice-bag), failed signally; and I hope that in some one at least of the large hospitals, cold-air rooms, in which the experiment in question may be fairly tried, will be established. As is well known, there is now no practical difficulty, except that of expense, in supplying such rooms, and therefore in thoroughly testing their therapeutical value as aids in the treatment of cholera. I believe they would be comparatively valueless without the simultaneous and persistent use of the spinal ice-bag; but seeing how *extremely* contracted the lungs are during collapse, and how the patients struggle for breath, I am led to think that cold used in the form of cold-air chambers, while ice is being applied to the spine, may prove extremely helpful in the treatment of choleraic collapse.

Of course such cold-air chambers could only be had in hospitals; but when they are not available cold can be applied externally in the form of cold affusions, followed by vigorous dry friction over the whole surface of the body several times a day. Dr. Fanton, of Marseilles, has described two cases in which rubbing the body all over with ice was followed by rapid reaction and cure.* Dr. W. C. Seaman, Deputy Inspector-General of Hospitals, says, "I was advised by the late Deputy Inspector-General of Hospitals, Dr. Archibald Stewart, to try the cold affusion, which, he said, had been used with success in Bengal." He followed the advice in 1859, and he says, "certainly with some success, inasmuch as the cramps were much relieved by it, and reaction to some extent obtained." In 1865 theoretical considerations caused him to add to this treatment the administration of ten or fifteen grains of calomel at the beginning of it, and he seems to think this was helpful; but while not endorsing his opinion on this point, I am grateful to him for repeating the experiment of the cold affusions, and

* *Marseille Médical*, Aug. 30, 1884, p. 497.

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especially for reporting the result, which certainly seems to have been satisfactory. He says, "The patient almost invariably felt relief; the cramps were mitigated, and sometimes in cases of collapse the pulse could now be just felt. In about five minutes the bath was repeated as before, after which, even in the worst cases, the pulse could now generally be felt; the skin became warmer, and the patient would beg for another bath. With reaction, vomiting and purging gradually ceased. The cold affusion was used at least five or six times in each case; and, as a rule, after each application of it the pulse grew stronger and all symptoms improved." * In seeming confirmation of Dr. Seaman's experience, I may report a fact communicated to me by a lady (the widow of a medical man). She lived many years in India, and while in Calcutta, about 1858, she was informed that a doctor there, whose name she mentioned to me, plunged a child, suffering from cholera, into iced-water after all the other treatment adopted had proved useless, and that the child recovered. One of my patients, an English clergyman, told me that when in Berlin some time ago, suffering from diarrhœa, he plunged head-foremost into a cold bath and then remained in it up to the neck during twenty minutes. He assured me that the diarrhœa quickly and completely ceased, and that he felt only "*the greatest comfort*" from the treatment. He has often treated threats of diarrhœa in like manner and with like success.

In my opinion the question whether it is wisest to apply heat or cold to the surface of the body generally while ice is being applied to the spine during collapse, is by no means settled. There seems *à priori* strong reasons in favour of both plans; those in favour of heat are fully stated in my book, and my patients recovered while the heat was used; those in favour of cold I have just given, and now it remains for experience on a large scale to dispel all doubt on the subject. Those physicians who have the opportunity of trying each plan have ample assurance in the facts I have mentioned that the use of cold in the manner described is neither dangerous nor disagreeable to the patients. It is especially worthy of remark that Dr. Seaman's choleraic patients "*would beg for another bath,*" and that my clerical friend suffering from diarrhœa "*felt only the greatest comfort*" from his cold plunge.

I have been recently informed by an eye-witness that at Malta, several years ago, choleraic patients were treated with wonderful success by the application of ice in India-rubber bags along the spine and *over the stomach* simultaneously.

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* "Cholera, its Etiology and Treatment." By W. C. Seaman, M.D. A letter published in the *Lancet*, July 19, 1884.

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SECTION XIII.

SUCCESS OF THE NEURO-DYNAMIC TREATMENT OF CHOLERA.

So far as I am aware, 41 cases of cholera had been partially or wholly treated by means of the spinal ice-bag previous to 1884; but of these 14 were so inadequately or so improperly treated that whoever is intent on forming a just estimate of the value of the remedial method in question must ignore them. Any one can verify the correctness of this assertion, for each of these 14 cases is reported in my book entitled "Cases of Diarrhœa and Cholera," where the numbers prefixed to them are as follows:—25, 27 to 32, 34 to 38, 40 and 41. But even of these 14 cases, defective as the treatment was, *eight were recovered from collapse.*

Of the 27 cases in which the treatment in question may be said to have been fairly carried out, a cure was effected in 24, so that 3 only, out of the 27, or 11 per cent., proved fatal. Of the 24 patients who were saved, 5 were in a state of partial collapse, and 16 were in complete collapse—several of these being quite pulseless, so that, of the whole of the cases reported, 5 were recovered from partial, and 24 from complete, collapse. The important significance of this statement will be at once understood by all who bear in mind the fact already adverted to—viz., that no drug yet discovered exerts any appreciable power in rescuing patients from the state of choleraic collapse.

Dr. Griffin, of Southampton, who had the care of several of the patients above referred to, summed up the characteristic features of the treatment in question in the following pithy sentence:—"*It stops cramps, vomiting, and purging; it makes the patients warm, and it prolongs life.*"

Of all diseases with which I am familiar, cholera is precisely the one in which the power of cold applied along the spine is most astonishingly manifested and is most astonishly rapid in its action. This truth is attested in the following extracts from the report of cases:—

CASE I.—The patient "agonized with cramps," suffering from extreme difficulty of breathing, and extremely anxious and restless, was laid on a spinal ice-bag, "*and within five minutes she was in a placid sleep.*"

CASE II.—Before treatment, cramps were occurring continually; the patient "had not had five minutes' sleep at a time for them." She was purged "every ten minutes or oftener." Her skin was cool, her

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head decidedly cold. The spinal ice-bag was first applied at 11.45 A.M. She was immediately soothed. At 2.45 P.M. the cramps were reported to be much lessened, and during the three hours' interval the bowels had been moved but twice. Before 7.15 P.M. of the same day the cramps had wholly vanished, and she had become "very comfortably warm all over."

CASE III.—At 4 P.M. the patient was in complete collapse: "lips blue, whole surface of body cold, cramps violent, rice-water purging and vomiting." The spinal ice-bag was then ordered to be applied continuously. In six hours afterwards, when she was next visited, she was "decidedly improved, markedly warmer, the cramps and sickness much lessened; *the cramps only recurred when the ice had melted and the spinal ice-bag had become warm.*" The next morning the lips were red, the whole surface of the body quite warm, the cramps had recurred only once, *and again only when the bag had been allowed to become warm;* bowels moved but three times in the twelve hours; vomited only once.

CASE IV.—At midnight the girl was in complete collapse; "rice-water stools; purging and vomiting almost incessant; very cold all over, tongue especially so; pulse almost wholly imperceptible; very bad cramps; countenance deathlike." Treatment was begun immediately. At nine o'clock the next morning she was already much better; she had vomited but once; was warm all over; the pulse was very distinct, 116; there were no cramps at all, and her countenance was "immensely improved."

CASE V.—At 11 A.M. the man presented all the symptoms characteristic of choleraic collapse. A spinal ice-bag was then applied; within an hour "a favourable change in him was observable;" he was already becoming warm; and at 9 P.M. of the same day, though still sick, he was "much better, warm all over, a capital pulse, no cramps."

CASE VII.—"A worse case of cholera," says the reporter, Mr. Peterson, who had seen many in India (where this occurred), he had "never seen." At 11 A.M. the man was suffering from "incessant vomiting and purging;" his extremities were "quite cold;" his pulse was "scarcely perceptible;" his voice "almost entirely gone;" he "was in great agony, and appeared to be sinking fast." Ice was applied along the spine, and within half an hour "I could perceive," says Mr. Peterson, "a change for the better. The cramps began to lessen visibly, and the involuntary purging to stop. He never vomited once after I applied the ice. I was much surprised, about half-past twelve, to find the pulse gradually getting stronger, and the legs and arms warmer. The warmth went on increasing until, at half-past one, his body, except where the ice was, became hot as in fever."

CASE VIII.—At 9 A.M. the girl was completely collapsed, and was quite pulseless. The spinal ice-bag was applied continuously, and by 2 P.M. of the same day reaction was perfectly established, "a good pulse" being observable.

CASE IX.—At 1.30 P.M. the girl was suffering from vomiting, purging, and cramps, and was collapsed and livid. The spinal ice-bag

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was applied continuously; the cramps and vomiting speedily ceased. By 5 A.M. the next day the purging had also ceased, and on the same evening she had "a good pulse," and was "warm all over."

CASE X.—At 10 A.M. the girl was suffering from vomiting, purging, and cramps, was quite collapsed, and quite pulseless. The spinal ice-bag was applied at that time, and continuously until 4 P.M., "when the countenance had become much less choleraic, and the vomiting, purging, and cramps were already greatly subdued."

CASE XI.—The woman was attacked suddenly in my presence. She became deathly pale, strikingly cold, the lips turned livid, and the sweat exuded in large drops over her face and upper extremities. Her head was cold and she was quite pulseless. *In about five minutes after the spinal ice-bag was applied,* "her pulse became distinctly perceptible, slight colour returned to her face, and in a few minutes more she said, 'I am better.'"

CASE XXIV.—"Was apparently hopeless."* The man was delirious owing to uræmic poisoning, caused by the prolonged arrest of the action of the kidneys. Within half an hour of the first application of the spinal ice-bag the patient was relieved, and, beginning at once to improve, continued to do so steadily until he had completely recovered.

That the foregoing summary of cases and results is not a pardonable exaggeration of facts due to the fervid enthusiasm of the writer will probably be held proven by evidence to which I shall now venture to refer. Nearly the whole of the cases in question occurred at Southampton during the epidemic there in 1865 and in 1866. Those medical men in Southampton who adopted or witnessed the treatment in question were neither using a remedy nor observing the effects of one originated by themselves; therefore their judgment of the value of that treatment was not likely to be either obscured or perverted. Now, at my request, they were good enough, in 1865, to express that judgment in letters from which the following passages are extracted:—

"I have now seen and treated, with Mr. Bencraft and Dr. Cheeseman, six cases of cholera, in the stage of collapse; and what I have witnessed and noted in these cases justifies me in stating that your treatment is superior to any other that I have hitherto seen practised or pursued by myself or others.

"Were I seized with cholera I should give your mode of treatment a preference; indeed, I would submit to no other."—JOHN WIBLIN, *Physician.*

"The five cases in which I had the good fortune of witnessing the application of the spinal ice-bags by you, have sufficiently convinced me of their utility in rousing the patients from collapse and removing the algide symptoms of cholera."—G. CHEESEMAN, *Physician, and District Medical Officer to the Southampton Incorporation.*

* This case was treated and reported by Dr. Allen, of Brighton.

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"Dr. Chapman's treatment of cholera proved itself a remedy of very considerable power, restoring the heat, relieving the cramps, checking the vomiting and purging. Its use was followed by reaction from collapse even in cases where the patient was quite pulseless.

"The treatment appears to have the great advantage of producing reaction from the state of collapse, leaving the patient free from the very large quantities of medicines which in other modes of cure so fatally hamper the treatment of the secondary fever."—G. A. LAKE, M.D., *Surgeon to the Royal South Hants Infirmary, &c. &c.*

"The power [of the treatment in question] to relieve the vomiting, purging, and cramps is almost marvellous, and its influence over the circulation, in restoring heat to the surface of the body, and in bringing back the pulse where nearly, and in some cases entirely, gone, must be seen in order to be properly appreciated.

"For my own part, I have such entire confidence in the method of treatment that, should any more cases of cholera come under my care, I shall without hesitation trust to it alone; and were I to be attacked with cholera I would insist on being treated entirely by your method."—HENRY BENCRAFT, M.R.C.S.A., L.S.A., *Medical Officer to the Southampton Workhouse.*

The efficacy of the treatment the results of which are described in the foregoing extracts, and which in the present state of medical science can be produced by no other means than those in question, would have received, in 1865, the authoritative attestation of the highest medical tribunal, if when I then visited Paris the celebrated Dr. Trousseau had happened to have charge of the cholera wards at the Hôtel Dieu. Before I left England he had expressed his readiness to afford me every facility he could for exemplifying at that hospital the treatment in question. Unfortunately, as I found on my arrival, there were no cholera patients in his wards; but he kindly introduced me to his colleague, Dr. Vigla, and requested him to accord me the opportunities which he himself was unable to give. Dr. Vigla placed two patients under my care, but a combination of adverse circumstances rendered any effective trial of my treatment at the Hôtel Dieu impossible. There was only one night attendant for a whole ward of cholera patients! And I was not admitted to the hospital between six at night and eight in the morning! Under such conditions the successful application of my method of treatment was impossible. Moreover, the lay director of the hospital was hostile to me; he announced to me that no new experiments could be tried, and that no new method of treatment could be permitted there unless first examined and sanctioned by a Commission; and then, in an imperative tone, ordered me to remove the ice-bag which was being applied. In consequence of the vigorous remonstrances of both Dr. Trousseau and

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Dr. Vigla, his attitude towards me certainly changed for the better; and I was invited to begin the treatment again. But the patients, having heard what the Director said, now refused to become the subjects of what he called "new experiments." Altogether, the difficulties I encountered, both at the Hôtel Dieu and at the Hôpital St. Antoine, were so insuperable that, utterly discouraged, I abandoned my attempt at that time to introduce my method of treating cholera in the Paris hospitals. If, I repeat, it had so happened that Dr. Trousseau had had charge of the cholera wards, he would, I have good reason for believing, have so seconded my efforts that the difficulties just mentioned would have been effectively counteracted, and he would then have pronounced a decisive verdict in favour of the treatment of cholera through the agency of the nervous system—a verdict which, coming with his justly great authority, would have settled the question. But, as events happened, the expression of this verdict had to be deferred nearly twenty years.

When cholera reappeared in Paris, in 1884, I called on Professor Peter, Physician to the Hôpital de la Charité, with whom I had the advantage of being already acquainted, and besought him to introduce me to some physician having the charge of cholera patients. He said, "Come to the Hôpital de la Charité." I went, and was delighted to find that he alone had charge of the cholera wards there. He immediately placed patients under my care, and I soon became thoroughly assured of his cordial co-operation in carrying out my treatment as effectively as possible. Moreover, the "interne of his service," Monsieur Carron, ably seconded his efforts. I was invited to enter the hospital whenever I pleased and to stay there as long as I pleased, both by day and night; and, a point of capital importance, on learning from me that it would be especially necessary to continue the applications of ice throughout the nights, an arrangement was made by which certain "internes" assumed in turn the duty of staying in the hospital each night in order to insure the re-application of ice every third hour. The aid thus rendered was invaluable; without it I should probably have lost several of the patients who were saved.

The following reports of the cases treated under the circumstances just mentioned are chiefly translated from the notes which were entered in the hospital case-book, and a copy of which was supplied to me by Monsieur Carron.

Of the twelve cases in question, those numbered respectively I. and II. proved fatal. Nevertheless, I invite especial attention to these two cases, for, though they were fatal, they exemplified in a striking degree the power of the special treatment adopted in subduing the symptoms characteristic of cholera. In Case I. the patient had suffered from what he called "gastralgia" during

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five years before he came to the hospital, and his tongue was thickly covered with fur—a condition never present in cases of uncomplicated cholera; and it is especially worthy of note that while even in this case, in which the algide condition, characterized by marked cyanosis and absence of pulse, was overcome—"the pulse being again distinct, the cyanosis diminished and the general temperature of the body having again become almost normal"—it was precisely the stomach disease of five years duration which seems to have been the cause of death; for of all the cases in question *this is the only one in which the vomiting was not arrested.* Again, in Case II., the patient, a washerwoman, is said to have been in the habit of drinking to excess. When treatment began she was suffering from vomiting, purging, cramps of the four extremities, and continuous thirst; there was marked cyanosis, and she was pulseless. She was speedily and completely relieved of all these symptoms; but after reaction set in she became fatally delirious—a condition likely to supervene during reaction in a cholera patient addicted to drink.

The patient referred to as Case III., the first cholera patient brought to the hospital (during the epidemic of 1884) and the last but one to leave it, was the only case already in the stage of reaction when my treatment of it began. It is interesting in this respect, and also especially on account of the character of the treatment of it which had been adopted during the algide stage. The first page of the chapter on Treatment, in my work "Diarrhœa and Cholera," discusses the possibility of treating cholera successfully by means of electricity, and I therein remarked that in my opinion, if it should prove available, the two conditions which would have to be fulfilled in order to insure its success are—"*first*, that the continuous current only be used; and, *second*, that its application be made to the whole spinal region." The reader will observe in this case that, animated by a like idea, Professor Peter made use of galvanism, and that, although the symptoms soon returned after each application, "each application was followed by immediate good effects."

Heat was applied to the upper third of the spine in this case for the purpose of stimulating the cervical ganglia of the sympathetic, in order, through their agency, to contract the cerebral arteries, and thus to lessen the existing cerebral congestion.

In Case IV. the symptoms, though comparatively slight, persisted several days, until at length it was proposed that ice should be used. Its good effects, as will be seen, were rapidly and strikingly evident.

CASE I.—C. E., male, aged 44, first seen Nov. 16, when he had been ill five or six days. Had been a sufferer from "gastralgia" during five years. His symptoms were vomiting, cyanosis; pulselessness, tongue loaded with fur. Since entering the hospital at noon of the 16th,

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up to six o'clock in the morning of the 17th, six applications of the spinal ice-bag were made. Owing to its influence the pulse returned, the cyanosis was lessened, and the general temperature of the body had become almost normal; but the vomiting during the same time did not appear to be modified—a fact due probably to the chronic disease of the stomach. The patient died at 9 P.M. Nov. 19.

CASE II.—Madame R., washerwoman, aged 53, entered the hospital Nov. 14. She was suffering from vomiting, diarrhœa, cramps of the four extremities; she had marked cyanosis, continuous thirst, and was pulseless. After four applications of ice, the vomiting, diarrhœa and cramps ceased, the pulse was restored, and the patient became warm.

In the evening of the 15th she became delirious to such an extent as to necessitate the use of the strait-waistcoat, and died Nov. 20.

CASE III.—C., female, entered the hospital Nov. 9; was treated from the 9th to the 15th by the continuous current—one pole being placed on the epigastric region, the other sometimes on the lumbar region, sometimes at the level of one pneumogastric. Each application was immediately followed by good effects in respect to the vomiting, the hiccup, and the cramps. But on each occasion the symptoms recurred shortly after the cessation of the electrisation.

I saw the patient for the first time on the 14th, and as she was in a somnolent state, verging on stupor, I applied a hot water-bag to the upper third of the spinal cord; a few hours afterwards it was observed that there was less cerebral oppression. By the 15th the patient was at the end of the algide period and reaction had set in; on the 16th the temperature was 100° F. But as the vomiting and diarrhœa persisted ice was used. Nine applications were made from the 15th to the 18th of November, and the vomiting and diarrhœa were then greatly lessened. The temperature oscillated between 103° and 104°; during three days it exceeded even 104°, and was accompanied by delirium. From the 18th to the 22nd of November the ice was omitted and the hot water-bag was applied, but irregularly, to the upper third of the spine.

Nov. 22. Bilious diarrhœa recurred, and the temperature began to lower. Three new applications of ice were then made.

Nov. 26. Diarrhœa completely ceased, and on Dec. 3 the patient left the hospital quite well.

CASE IV.—L., male, admitted Nov. 13; ill since the 12th. Moderate vomiting; no cramps nor hiccup, but abundant diarrhœa, for which ice was applied five times.

The diarrhœa then disappeared during three days. Afterwards a few diarrhœic stools occurred, but so irregularly and so insignificant, that it was not thought necessary to repeat the application of ice. An abundant polyuria (5 to 8 quarts) with albuminuria and glycosuria, began Nov. 21. The presence of albumen and sugar in the urine continued only three days, but the quantity of the urine remained very abundant until the patient left the hospital otherwise quite well.

CASE V.—C. D., male, aged 26, entered the hospital Nov. 10,

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when he was suffering from bilious diarrhœa without either cramps or vomiting. Thirst moderate, urine small in quantity, in a word, symptoms of simple cholerine. The next day there was a notable improvement, and the patient asked for food.

Nov. 13. Vomiting came on with rice-water stools, pulse small, voice almost extinguished; urine loaded with albumen; hiccup.

Nov. 14. During the night between the 13th and 14th there was an eruption of urticaria over the whole body, each patch of which was surrounded by a violet ring. Since the evening of the 13th the urine had been suppressed. The first application of ice was made on the morning of the 14th.

Nov. 16. Nine applications were made successively every third hour, and on each occasion was followed by an amelioration in respect to the vomiting and hiccup. At this date the improvement was very pronounced. The vomiting and diarrhœa had ceased. The urine reappeared and contained a small quantity of sugar and albumen.

Nov. 22. The patient left the hospital cured.

Case VI.—L. S., aged 28, entered the hospital Nov. 13; ill since the 12th. Diarrhœa (involuntary liquid motions of a dark brown colour), slight pulse, hiccup, suppression of urine, cramps of the four extremities; pronounced cyanosis. Seen for the first time by Dr. Chapman on Nov. 14: his pulse had then become quite imperceptible. Ice was first applied along his spine at 11.30 A.M., and was renewed every third hour throughout the day, at the end of which he had become much warmer; the vomiting, diarrhœa, hiccup and cramps had disappeared, and the pulse had returned.

Nov. 15, 5 P.M. The patient, who had not passed urine since he entered the hospital, said he wanted to make water but could not do so. The bladder was distended; the passage of a sound was prevented by a rigid spasm of the neck of the bladder. The patient was therefore put into a warm bath, in which he urinated twice.

Nov. 16. The use of ice having been unintentionally omitted from 3 A.M. of the 15th until 10 A.M. of the 16th, bilious vomiting recurred. Ice was then applied again and was renewed every third hour. In the course of the day the vomiting again ceased, and recurred only once in the evening after the patient had drunk a quantity of milk and water.

Nov. 22. After five additional applications of ice, begun on the 16th and ended at 3 o'clock in the morning of the 18th, he passed on the 16th a great quantity of urine, and on the 17th, 18th, and 19th, from 3 to 4 quarts each day. The urine contained both albumen and sugar. The patient complained only of a little epigastric pain; and at this date, the 22nd, he left the hospital completely cured. He expressed himself especially grateful for the treatment he had received, and added emphatically, "La glace m'a sauvé."

Case VII.—G., aged 63, male, first seen Nov. 14; ill during about ten days previously. When he entered the hospital he was suffering from incessant vomiting and diarrhœa, hiccup, and sup-

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pression of urine. The ice-bag was immediately applied along his spine; he experienced immediate and great relief. The hiccup disappeared and the patient slept half an hour on the ice. The hiccup returned several times during the night; three applications of ice were made, one every second hour, and they were each time followed by the rapid cessation of the hiccup, which afterwards did not return till the 16th. After the third application of ice the vomiting ceased.

Nov. 16. The patient began to urinate; the urine contained both albumen and sugar. He complained of pain at the level of the insertions of the diaphragm. At this date he refused further application of ice. The vomiting and hiccup recurred, but not the diarrhœa. A blister was applied over the epigastrium and he was given a glycerine enema.

Nov. 22. The urine (3 quarts) still contained a little sugar, but no albumen. The patient left the hospital at his own request.

Case VIII.—Madame G., aged 21, in her seventh month of pregnancy; first seen Nov. 15 when she was in a state of collapse, with vomiting, diarrhœa, cramps and coldness of the surface of the body. After the fifth application of ice, the vomiting and cramps ceased, the diarrhœa was lessened, and she had become notably warmer. Then the two upper cells of the ice-bag were applied to the upper two-thirds of the spine; two applications were thus made. She then refused the further use of the ice, notwithstanding the recurrence of vomiting (of a greenish hue).

Nov. 23. Since the discontinuance of the ice she was subjected to no other treatment; nevertheless, the improvement already effected continued and increased, and the patient left the hospital cured at this date.

Case IX.—Madame Josephine O., aged 43, entered the hospital Nov. 16, ill since the 13th; she suffered from cramps, diarrhœa, epigastric pains and suppression of urine: her voice was feeble, pulse very weak; cyanosis of lips and extremities. Immediately after she entered the hospital ice was applied.

Nov. 20. Three applications of ice were made. The patient urinated, and the diarrhœa, cramps, and cyanosis disappeared, but she complained of sleeplessness. A spinal water-bag containing water at about 115° F. was applied along the upper third of the vertebral column; and a draught containing 15 grs. of bromide of potassium, 15 grs. of bromide of ammonium, and a drachm of tincture of calumba was given in the evening.

Nov. 26. The patient continued to improve, although at intervals she had a slight diarrhœa—so slight that it was not thought expedient to arrest it by fresh applications of ice. At this date she left the hospital cured.

Case X.—Mlle. B., a wet-nurse, aged 23, entered the hospital Nov. 16, at 10 P.M., when she was suffering from incessant vomiting and purging, cramps and hiccup. The body was cold, her hands and nose especially so. The urine was suppressed, and the pulse imperceptible. Ice was applied immediately. After the third application the amelioration was very striking. The vomiting and

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purging recurred only at distant intervals, and the pulse had returned.

Nov. 17, 5 P.M. The improvement continued; the vomiting had not recurred. She was troubled only with a slight intermittent hiccup. On this occasion she said, "*Le sac à glace est mon sauveur.*"

Nov. 18th, 6 P.M. The improvement was maintained; the return of bodily warmth was complete; vomiting had not recurred, but the bowels had acted four times before 4 P.M. As her breasts were distended with milk, the application of ice was from this time limited to the lower two-thirds of the spine.

Nov. 19, evening. The diarrhœa had completely stopped. The urine, which returned in abundance on the previous day, contained both albumen and sugar. Ice was applied for the last time at this date, twelve applications having been made altogether, and the patient continued to improve rapidly.

Nov. 22. The patient left the hospital perfectly well, except that the urine still contained a trace of sugar.

CASE XI.—D., male, aged 50, entered the hospital Nov. 18; he was attacked with diarrhœa on the 14th, but was able to work a little until the 17th. When he entered the hospital he was suffering from abundant vomiting and diarrhœa, cramps, and the suppression of urine, since the 15th; voice nearly inaudible. The expired air cold, pulse thready.

Ice was applied immediately at 9.30 P.M.; three additional applications were made during the night.

Nov. 19. Neither vomiting nor diarrhœa during the night; this morning he vomited once very copiously. The pulse was better; the ice was continued.

Nov. 20. Up to this morning ice had been applied ten times. The patient had a good night, and passed a little urine, but still vomited copiously. In the expelled liquid there floated dark-coloured matter.

Nov. 21, morning. Three fresh applications of ice were made yesterday, and three during the night. Is now very much better: neither vomiting, nor diarrhœa, since last evening. The urine was loaded with albumen.

Nov. 22. The seventeenth and last application of ice was made at six o'clock the previous night. Condition of the patient excellent. He was taking food. The urine (3 quarts) still contained albumen and a little sugar.

Nov. 23. The improvement continued. The urine (3 quarts) still contained albumen, but no sugar.

Nov. 24. The patient left the hospital cured.

CASE XII.—B., aged 30, entered the hospital Nov. 19; suffering from diarrhœa since the 16th. Since the 18th suffering from bilious vomiting, rice-water stools, cramps, and suppression of urine. The body was moderately cold, the pulse small. The first application of ice was made at 11.30 A.M. Three additional applications were made during the day, and two during the following night.

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Nov. 20. The vomiting ceased at one o'clock this morning, and the bowels acted only twice since yesterday. The patient had become warm.

Nov. 21. The patient passed urine for the first time, it was loaded with albumen.

Nov. 25. The improvement continued, no return of vomiting or diarrhœa; two quarts of urine were voided daily with traces sometimes of sugar, sometimes of albumen; pulse had become thoroughly good, and the patient left the hospital at this date cured.

The proportion of deaths to recoveries in the immediately foregoing cases, two out of twelve, or 16·6 per cent., approaches nearly to that recorded in the account given at page 106 of my previous treatment of the disease—viz., 11 per cent.—and can scarcely fail to impress every one familiar with the history of cholera as very remarkable.

It has indeed been alleged as a ground for discrediting the efficacy of the therapeutical method in question, that the cholera which prevailed in Paris during November and December, 1884, was of an especially mild type; and that the cases received into the Hôpital de la Charité were less grave than those received into the other Paris hospitals. The *Journal of the American Medical Association*, in its number published March 28, 1885, contains a report of a paper concerning my etiology and treatment of cholera, read to the Suffolk District Medical Society, by Dr. B. O. Kinnear of Boston, U.S.A., and of the debate which that paper originated. The general tenour of the opinions expressed by those who took part in that debate was to the effect that, as expressed by Dr. Amory, "the disease was developed only in a very mild form;" Dr. Blodgett, "who spoke of the epidemic in Paris, of which he had gained some knowledge by personal experience," said "the disease was certainly there regarded as of very mild type and the rate of mortality was exceptionally low."

Gentlemen who make general statements of this kind unsupported by duly authenticated facts, easily gain a cheap sort of reputation for superior wisdom, especially if they fringe their assertions with oracles like those uttered by Dr. Edes, the chairman of the meeting in question, who said—"The tendency to formulate attractive theories of treatment in very obscure diseases is always liable to lead to erroneous deductions, and all investigations of this character should be conducted with more freedom of judgment and absence of prejudice than an enthusiastic advocate of a new idea is liable to possess."

But to enable my readers to estimate correctly the value of the allegations just mentioned, I proceed to furnish them with some "duly authenticated facts," bearing on the question at issue.

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In Professor Goodeve's important article on Epidemic Cholera, published in Reynolds' "System of Medicine," there is a section headed "Mortality of the Disease," which contains the following statements:—"It has been observed for England, that whether in healthy or unhealthy districts *the deaths to cases are pretty even*. . . . . We may say in round numbers that *more than 50 per cent. of the attacked recover*. In some epidemics the mortality is higher, but in others lower: in some 70 to 80 per cent.; in others 20 to 30 per cent. . . . . In 1832, in England the deaths to cases were 47 per cent.; in 1848–9, 45 per cent., and in 1553–54, 46 per cent." Professor Goodeve gives abundant statistics of the mortality in India, but I do not cite them because experience of different epidemics in England offers a more trustworthy standard of the ordinary mortality from cholera in Europe than Indian experience can furnish. It appears, then, according to actual experience of several epidemics, that "in round numbers more than 50 per cent. of the attacked recover." Now, how does this accord with the recorded experience of the epidemic in Paris in 1884? On the 9th of December of that year M. Dujardin-Beaumetz communicated to the Academy of Medicine the result of his statistical inquiry on the subject. He showed that, while both at Toulon and at Marseilles the mortality had been 50 per cent. of the cases and at Nantes 51 per cent., at Paris the deaths in proportion to the cases were 54·47 per cent.! He says, "According to the hospital statistics ascertained by M. Emile Rivière, there were, from the 4th of November to the 1st of December, 1,037 entries and 564 deaths. This mortality is at the rate of 54·47 per cent." Referring to the statistics of mortality during previous epidemics in France, M. Dujardin-Beaumetz observed,—"All these figures are almost identically comparable *and thoroughly show the morbidity of this epidemic* [of 1884], which has been the same at the different points where it impinged; that which distinguishes these different epidemic foci is not the [relative] gravity of the cases, but the number of the persons attacked."

Referring to the allegation that the cases received into the Hôpital de la Charité were less grave than those admitted into some of the other Paris hospitals, I may be permitted to remark that, at all events, it is not probable that the cases confided to me for treatment by way of testing the value of my method were, on the average, less grave than those treated simultaneously in the same hospital in the manner ordinarily practised there. Now, 31 cases were thus treated, and of these, 15 or 48 per cent. proved fatal. If the average character of the cases treated according to each of the two methods respectively was exactly similar, the much greater proportion of cures effected

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by the neuro-dynamic method should, I presume, be ascribed to the much greater efficacy of that method. I venture to add here the expression of my belief, that if adequate attention could have been given to Case II. the patient might have been saved; but the amount of aid in the shape of nurses, especially at night, was so small as to render such adequate attention quite impossible. On this subject I beg to recall the reader's attention to my remarks in Section XI.

It appears, then, that the epidemic of 1884 in Paris, judged by the number of deaths in proportion to the number of persons attacked, was rather more severe than epidemics of cholera in Europe usually are, the mortality being 54½ per cent.; and that though the cases in the Hôpital de la Charité seem, judging from the mortality—viz., 48 per cent. of those treated by the ordinary method practised there—to have been somewhat less than the average proportion of deaths to cases in all Paris, yet they were as severe as, according to Professor Goodeve, the great majority of cases of cholera usually are; and, certainly, I do not hesitate to affirm that the cases treated by me were quite as severe as those of which 48 per cent. proved fatal. Of my cases, as already stated, two out of twelve—or 16 per cent.—were lost. Of the 27 cases which I had previously treated, three were lost. If the results of these experiences be added together, it appears that 39 cases have been treated by my method, and that five—or 12·8 per cent.—proved fatal.

The histories of the twelve cases treated by me in Paris are very brief, and far from complete; but such as they are, they will, I believe, if duly examined, yield some valuable instruction, as well as some interesting confirmation of the etiology of cholera explained in the first part of this essay.

It is worthy of note that though Case IX. was distinguished by several of the most characteristic features of cholera—viz., cramps, epigastric pains, diarrhœa, suppression of urine, partial aphonia, very slight pulse, and cyanosis of lips and extremities—there was neither vomiting nor nausea. On the other hand, in Case I. there was vomiting, cyanosis and pulselessness, but no diarrhœa; and in Case VII. there was persistent hiccough, incessant vomiting, and suppression of urine, but no diarrhœa. Cramps are recorded to have been complained of in seven out of the twelve cases. In five cases hiccough was experienced. In four cases there was total absence of pulse (which in each case was restored), and in one it was thready and scarcely perceptible. In seven cases the urine was albuminous, was saccharine, and was suppressed; and in four there was marked polyuria. Considering this brief summary of the symptoms actually experienced, we see that in cases of equal,

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or nearly equal, gravity the several groups of symptoms presented by the different cases differ remarkably from each other, and therefore from that classical aggregate of symptoms commonly described as characteristic of a typical case of cholera.

This difference in the several groups of symptoms in different cases I explain by assuming that the morbidly hyperæmic states of the several parts of the spinal and sympathetic nerve centres differ in relation to each other differently in different cases. For example, in a case in which there is persistent vomiting, but no diarrhœa, the nervous centres functionally related to the stomach and to the muscles concerned in vomiting are morbidly affected to a much higher degree than are those related to the bowels; whereas, conversely, when diarrhœa is a prominent and persistent symptom, vomiting being absent, the nervous centres related to them are morbidly affected to a much higher degree than are those related to the stomach. Now, when this is the case, the bowels, by prolonged action, are emptied of the whole of their ordinary contents, and just as in very severe cases of sea-sickness, after the whole of the food contained in the stomach has been ejected, mucus is secreted abundantly and retching persists, so in the bowels mucus is secreted abundantly, and constitutes the main element of the dejections—the "rice-water stools." In cases in which the nervous centres functionally related to the bowels produce these results, the mucous membrane of the bowel has become so preternaturally active as to approach the state of inflammation—a state *especially favourable*, as shown by Professor Horsley's experiments on the monkeys mentioned above, for the production of comma-bacilli. This view of the facts receives striking confirmation from one of the results of the researches respecting cholera in 1884 by the Marseilles Commission, which in its report says,—"One fact had struck us from the beginning—viz., that there is constantly an inverse relation between the proportion of comma-bacilli and the coloration of the stools: that is to say, that the rice-water stools are those which contain the most, whereas we have examined coloured stools which contained none whatever." *

The views expressed above—viz., that different parts of the nervous system are morbidly affected to a maximum degree in different cases—explain the fact pointed out by Dr. Poucel, who says, "From a clinical point of view there is no relation between the gravity of cholera and the coloration of the stools. The most characteristic specimen of rice-water stools rich in comma-bacilli that we have seen were from a patient whose symptoms were light, and who recovered." †

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* *Marseille Médical*, Oct. 30, 1884, p. 577. † *Ibid.* p. 616.

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The notes of these cases do not record what was the relative rapidity with which the vomiting and diarrhœa were stopped by means of the spinal ice-bag. Probably the question is of but little importance, especially as these disorders differ greatly in relative severity in different persons; I may mention, however, that according to the reports of ten out of the first eleven cases given in my work, "Diarrhœa and Cholera," vomiting was arrested first in three cases, diarrhœa was arrested first in two cases, and in five cases the two disorders were arrested simultaneously.

Having now described the results of the treatment which is the direct logical outcome of the pathological principles sketched in the first part of these pages, and having adduced the important testimony given above concerning these results, I appeal to all competent and impartial judges to pronounce whether, inasmuch as ice applied along the spine "stops the cramps, vomiting, and purging, makes the patients warm," and prŏduces "reaction from collapse, even in cases where the patient was quite pulseless," its action does or does not constitute a practical verification of the doctrine I have propounded—viz., that "the *proximate* cause of all the phenomena of cholera [before the stage of reaction] is hyperæmia, with consequent excessive action, of the spinal cord and of the sympathetic nervous system." What is the physiological action of ice? It is powerfully sedative. Obviously, therefore, if the exertion of a powerfully sedative influence over the spinal cord and collateral ganglia of the sympathetic nerve abolishes the phenomena of cholera, it must do so by lessening the circulation of blood in, and, consequently, the energy of, those nervous centres. But if it does this in cases of cholera, then it follows that the essential condition precedent of recovery from the disease is diminution of circulation in, and consequent energy of, those nervous centres, and therefore that the proximate cause of the disease (however that cause may have been established) consists in "hyperæmia of the spinal cord and of the sympathetic nervous system."

SECTION XIV.

The Analogy between Cholera and Sea-sickness.

If the symptoms of cholera were the consequence of a blood-poison, or of a microbe in the intestines, they, surely, could not be subdued with the rapidity exemplified in many of the cases above referred to by the use of a purely dynamic agent—the

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application of ice along the spine. It would, in my opinion, be just as rational to ascribe the several phenomena of sea-sickness to one or the other of those two alleged causes, as it is to maintain that either of them is capable of producing the multiform symptoms of cholera. As I have already said, the analogy between the two diseases is strikingly great. There are various predisposing causes both of sea-sickness and of cholera undoubtedly inherent in different individuals—otherwise of, say, 100 persons placed in the same locality and under the same circumstances in a cholera-stricken city, and of 100 persons on board the same ship, the whole would either become victims of cholera and of sea-sickness respectively, or the whole would escape. But we know that, as a matter of fact, only a certain proportion suffer, and these in different degrees, while the rest are not affected at all. The ultimate cause of sea-sickness—motion—operates alike on the whole 100 passengers; but a certain number resist its influence. And in like manner the ultimate cause of cholera—whether thermal, electric, or (as I believe is most generally the case) thermo-electric, each of which is a correlative of motion and convertible into it—operates alike on all within the several foci of its energy; but just as a certain number resist the influence of motion and remain free from sea-sickness, so a certain number resist the influence of heat or electricity, and, though dwelling in a cholera district, remain proof against cholera. The essential likeness to each other of these two diseases is still more strikingly attested by the fact that they are both curable by one and the same method. Now, seeing that in very severe cases of sea-sickness, nearly all the symptoms of cholera, including even those of diarrhœa and sudden onset, are often manifested; seeing that the cause of sea-sickness is motion, and that the ultimate causes of cholera are heat and electricity, which, as physicists teach us, are modes of motion; seeing that the personal predisposition to each disease is analogous; seeing that each is a disorder of the nervous system; seeing that each is curable by acting in one and the same way on that system; and, seeing that we are absolutely certain that one of them, sea-sickness, is not the result of a blood poison, is not the result of a microbe, and is not contagious, we must, I think, conclude that the other is not less free from these characteristics, and not less a purely dynamic product than is sea-sickness itself.

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SECTION XV.

Public Declaration in the Paris School of Medicine that Cholera is a Disease of the Nervous System, and that the Neuro-Dynamic Treatment of it is Successful.

The first edition of my book on Diarrhœa and Cholera was published in 1864, and the second in 1866. The hypothesis expounded in them was cordially appreciated by the *Medical Press and Circular*, the *Medical Times and Gazette* on two separate occasions, and the *Journal of Mental Science;* but, so far as I know, by no other medical journal.* As, however, at least three of the writers of those reviews are eminent men—viz., Dr. Clifford Allbutt, Dr. Maudsley, and Sir Andrew Clark, whose judgments outweigh those of a legion of ordinary medical critics—the encouraging recognition which they accorded the work increased my confidence that time only was needed to produce in the minds of the members of the medical profession generally a conviction of the truth of that hypothesis. Moreover, by the success which attended my practical application of it at Southampton, and the impression which the effect of the treatment made on the medical men who witnessed it there, this confidence was greatly strengthened. At length, the doctrine for the establishment and diffusion of which I have striven during twenty years, and which during that time has remained in comparative obscurity, has emerged into the full light of day: both it and the results of the treatment which is the logical outcome of it have now received authoritative and public recognition in the first medical school of Europe by one of its most distinguished Professors—Professor Peter—who, having himself given especial attention to the pathology of cholera, and having watched my treatment of it in the hospital wards under his own control, is peculiarly qualified to pronounce judgment concerning the effects of that treatment as well as concerning the doctrine on which it is founded. That judgment is recorded in the following extracts from Prof. Peter's lectures (being a part of his Course on Pathology), delivered Dec. 9 and 11, 1884, at l'Ecole de Médecine, Paris:—

"Observation teaches us that in the different forms of cholera the great sympathetic nerve is profoundly affected. Is this

* Indeed, one of the chief medical journals, professing to review the second edition of the work, dismissed it with a few contemptuous sneers.

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alteration primary or secondary? My learned friend Dr. Chapman thinks there is evidence of a primary and simultaneous hyperæmia of both the spinal cord and the ganglia of the great sympathetic. This is a judicious idea, which affords a satisfactory interpretation of the facts. That the great sympathetic is morbidly affected is indubitable." Referring to a *post-mortem* examination which had been made the day before, and which showed that the ganglia of the solar plexus were congested, the Professor said—"You see, gentlemen, that the most minute examination, made by a man accustomed to these researches, shows us that there is hyperæmia but without organic lesions. *It is precisely because there is hyperœmia only that the patient may be cured if the treatment aims at this phenomenon.* . . .

"*I insist on this absence of all organic change, for there is the knot of both the pathological and the therapeutical problem. Whatever may be the form of the disease it consists wholly in profound but curable perturbation of the nervous system.*

"Serous cholera borders closely on algide cholera, and in both these forms we ought to intervene in a similar way. It is here that we shall obtain very remarkable results from the treatment originated by my learned friend Dr. Chapman, who has made a very profound study of cholera. He sets out with the idea that the troubles of cholera are due to hyperæmia of the spinal cord, and of the great sympathetic. And, to combat this condition, he applies along the vertebral column a bag filled with pounded ice. The consequence is a refrigeration of the skin, and of all the subjacent tissues; in other words, first physical action, then dynamical action. The refrigeration is continuous and permanent, for at every third hour the bag is refilled with pounded ice. There is no doubt but that which takes place in the surface of the skin also takes place in each successive layer of the underlying structures, the spinal cord and nerves emanating from it inclusive.

"*The results of the application of the ice-bag are very remarkable.* Suppose there be hiccough: it is almost immediately lessened, then suppressed. What is hiccough if it is not a spasmodic convulsion of the diaphragm of *medullary* origin? The vomiting was almost always diminished, then stopped. If it recurred, a fresh application of ice again arrested it. Cramps are to the extremities what hiccough is to the diaphragm, and they also are advantageously modified by the same treatment.*

"The diarrhœa appeared to be equally lessened, but the amelio-

* The foregoing record of the cases referred to by Prof. Peter shows that in each case in which cramps were experienced they were rapidly and completely arrested.

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ration in this respect was less striking, for the arrest was more slowly effected;* whereas the disappearance of the other symptoms was rapid. In several cases in which the application of the ice was discontinued too soon, the vomiting reappeared, but the treatment being renewed, was quickly followed by cessation of the trouble.

"*It must be borne in mind that this treatment, which is admirably simple and inexpensive, is within the reach of everybody.*

"I had a similar idea a long time ago: if, I said to myself, I should have to treat cholera patients, I would combat the irritation of the solar plexus by the continuous current. One pole applied to the pit of the stomach, and the other either over the spinal cord or at the level of the pneumogastric nerves, ought to modify the innervation of the morbidly affected sympathetic nerve. This treatment is rational, for it calms the nervous system by the best nervous sedative—viz., the continuous current. I have tried this treatment, and have obtained by means of it results like to those obtained by the use of Dr. Chapman's ice-bag. But the effects were less durable; the hiccough disappeared immediately, to reappear shortly afterwards; moreover, the application of the continuous current is a difficult affair, and not easily practicable; first, a galvanic pile is necessary; second, a pile which will work (a rare thing in a hospital); and third, a pile for each patient. The ice-bag has the advantage of being more practicable, and in a matter of this kind the simplest is the best. *I cannot advise you too strongly to make use of the treatment by ice, according to Dr. Chapman's method,* for its application is very simple, and it produces good results.

"I have adopted Dr. Chapman's treatment in a certain number of cases of cholera at l'Hôpital de la Charité. Twelve patients have been treated exclusively by this method: of these ten were cured and two died; one of these two was a man who had been, according to his own expression, a 'gastralgic' during the five or six previous years; the other was a woman who had been an habitual drinker. In short the proportion of deaths was 16 per cent. The total number of cholera cases under my care was 43. Of these, 17, or 39 per cent., proved fatal. This proportion of deaths is, you see, much below the average.

"Dr. Chapman's ice-bag is made of india-rubber, and consists of three pockets, placed one upon another, each of which is filled separately with ice broken into very small fragments. The bags intended for adults are from 20 to 26 inches long. The bag is applied along the vertebral column, on to which it easily moulds

* See, in respect to this point, the statement at page 120.

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itself. Its application should be continuous; it is therefore filled afresh every third hour. The number of applications in the different cases varied from three to sixteen. *The effects produced were marvellous.* Immediate relief was experienced. 'The ice-bag is my saviour' (*le sac à glace est mon sauveur*) was the exclamation of one of the patients. In two cases, a continuous hiccough, lasting several hours, disappeared within ten minutes after the first application, and the patient slept on the ice.

"*From the first application there was a notable diminution of the vomiting, of the epigastric pain, and of the cramps. The patients became warm and the pulse perceptible.* This modification of the epigastric pain is interesting; for the ice-bag is far from the epigastrium, but it exerts its influence by modifying the innervation of the great splanchnic nerves. In respect to the cramps the same thing occurs. *To say that ice applied to the bodies of the sufferers, already algide (almost icy-cold), warms them, is a paradox, and nevertheless nothing is more true. In fact, by putting an end to the spasm of the blood-vessels, the blood is allowed to circulate, and consequently the animal heat is generated afresh.*

"In short, ice applied along the spine, and the continuous current, produce similar results; but the effects of the ice are more durable than those of electricity. This superiority is due to the prolonged application of the ice, whereas the continuous current was only applied during two or three hours each day.

"THE BASIS OF RATIONAL TREATMENT OF THE ALGIDE STAGE CONSISTS IN MODIFYING THE CONDITION OF THE NERVOUS SYSTEM."

SECTION XVI.

SUMMARY AND CONCLUSION.

I HAVE now laid before my readers a view of the nature, causes, and treatment of cholera, which differs fundamentally from that generally accepted, and indeed from each and all previously promulgated. This distinctive feature may, perhaps, be regarded as *primâ facie* evidence by no means in its favour, and might be justly so regarded were not the pre-existing doctrines and practices mutually irreconcilable, or were any one of them capable of offering explanations, at once intelligible and consistent with each other, of the whole phenomena of cholera, and also capable of suggesting a method of treatment at once scientific and successful. But, as a matter of fact, the whole litera-

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ture of cholera, exceptions excepted, is a literature of chaos: a chaos of conflicting doctrines, which severally seem capable of explaining some of the phenomena of the disease, but which fail signally to explain the whole; and consequently a chaos, not less confounding, but far more dangerous, of conflicting practices. On the other hand, the doctrine now submitted to the reader offers a simple and clear explanation of every symptom and every characteristic of cholera, and though these symptoms and characteristics are, in the aggregate, very numerous, and, therefore, necessitate numerous explanations, yet the whole of these are absolutely accordant with each other. While cholera is shown to be essentially a disease of the nervous system, the genesis of every symptom of the disease, or its mode of origin in that system, is distinctly demonstrated. Though the factors in the creation of that special condition of the nervous system out of which they arise are numerous and manifold, the hypothesis in question assigns to each predisposing or exciting cause its appropriate place and function, and reveals its special mode of action as a co-operative agent. The increase of heat immediately before death, the increase and persistence of heat as well as the remarkable changes in the colour and aspect of the visage and general surface of the body, together with the not infrequent movements of the limbs after death, are simply and satisfactorily explained by the same hypothesis. It renders completely intelligible the established fact that cholera often arises suddenly in numerous localities, more or less distant from each other, at one and the same time, and that occasionally it ceases no less suddenly. While recognizing, and fully accounting for, the facts out of which the doctrines of a "cholera-poison," of "cholera-germs," of "cholera-bacilli," and, therefore, of the contagiousness of cholera have arisen, the hypothesis now propounded at once repudiates and confutes those doctrines, and at the same time, by proving that cholera is exclusively a dynamic disorder, also proves that it cannot be contagious. In doing this it justifies Anglo-Indian physicians who, holding the disease to be non-contagious, treat patients suffering from it in hospital wards along with those afflicted with other diseases. It explains the general immunity from cholera of the attendants of cholera patients, as well as of students who dissect cholera corpses. It explains why, notwithstanding that animals are liable to cholera as human beings are, Dr. Koch could not induce the disease in them, however much he fed them with his comma-shaped microbes or with portions of intestines containing them. It also explains why quarantine in all its vexatious and misery-inflicting forms, the disinfection of travellers, and of goods and letters *in transitu* are useless and indefensible as means of protection against cholera.

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It shows that the summer diarrhœa of Europe, the *cholera infantum* of the United States of America, the diarrhœa premonitory of cholera, cholera *nostras*, and "Asiatic" cholera are all mere varieties, or grades of intensity, of one and the same disease, that they are all alike due to the same *proximate* cause, and are controllable in one and the same way. Finally, it dictates a method of treatment which is approved alike by medical science and by common sense, which is thoroughly practicable, which is sanctioned by the experience of every physician who has adopted it, and which, fulfilling the promise implied in the title-page of this essay, proves cholera *curable*.

THE END.

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REVIEW BY

SIR ANDREW CLARK, BART., M.D.,

OF THE SECOND EDITION OF

DR. CHAPMAN'S WORK ON

DIARRHŒA AND CHOLERA,

Published in 1866,

Reprinted from "The Medical Times and Gazette," of November 3, 1866.

"This is a remarkable book, and worthy the serious attention of every one of our readers who has the leisure and inclination to think out his own opinions upon an interesting and difficult subject. We do not say that the theory of cholera which the author proposes is correct, or that the treatment based upon it is sound. Such an admission is not necessary to justify our recommendation. Even a false theory when rightly constructed has its uses, and, instead of hindering, hastens the advance of knowledge. Every one possessing the slightest acquaintance with the history of astronomy knows that the doctrines of cycles, epicycles, and ellipses were begotten naturally and necessarily out of each other, and that if Kepler had not so often propounded speculative errors, Newton would not so often have hit upon speculative truth. When men of science disclaim hypotheses, they are either unfit for their vocation, or, like Newton, they are better than their creed. Hypotheses are at once the effects and causes of progress; and one might as well attempt to preserve and employ an army without organization, as to preserve and employ phenomena without a theory to weld them into one. But the theory must be provisionally, if not positively, true; it must be intelligible and consistent; it must explain a greater number of facts and reconcile a greater variety of apparent contradictions than any which has preceded it, and it must have become developed, not by the addition merely, but the addition and solution, of subsidiary explanations.

"Now the book before us contains a speculation which, whether true or false, exhibits in its outlines, if not in its details, the essential features of a well-constructed theory. It is in harmony with the results of the most recent physiological investigations; it is ingenious, clearly put, happily illustrated, logically argued, and meant to be a more comprehensive and simple explanation than has yet been given of the phenomena of choleraic disease. For these reasons we have considered it our duty to place at once before our readers a sketch of Dr. Chapman's work, and to give judgment on its general character.

"The introduction to Dr. Chapman's book is occupied by a sketch of his views of neuro-physiology, and as they properly constitute the root of his pathology, it is necessary to consider them. He sets out with two postulates—first, that the sympathetic is the excito-motor nerve presiding over the vascular system; and secondly, that the functional activity of every gland is excited or maintained by a stimulus sent from the cerebro-spinal axis. The former doctrine, which derives its claims to assent from the experiments of Brown-Séquard and Claude Bernard, is now, with certain qualifications and restrictions, accepted as true by nearly all physiologists. The latter, based upon the recent discoveries of Bernard, Ludwig, Pflüger, and others, has, as Dr. Chapman thinks, being raised to the rank of a general law by his own observations.

"Bernard has proved that the parotid and submaxiliary glands receive their nervous supplies, on the one hand from the cerebro-spinal system, and on the other from the sympathetic; and he has demonstrated by experiments on various animals

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that when the former are in action the maximum of blood is supplied to the glands and the maximum of saliva secreted, and that when the latter are in action it is shown in modifying the volume of the arteries and so regulating the supply of blood. Dr. Chapman has extended this discovery to all the other glands of the body; but in virtue of some experiments about to be mentioned, he puts an interpretation quite different from that of Bernard upon the purpose of the cerebro-spinal gland nerves. The latter conceives that they act by producing a kind of paralysis of the sympathetic; the former that they act by stimulating the gland-cells to the active selection from the blood of the materials essential to the elaboration of their secretions.

"According to Dr. Chapman's statements, he has discovered that the application of heat along the spine stimulates the glands of the whole cutaneous and mucous surfaces, and that conversely the application of ice restrains or depresses them; that in the former case the secretion is increased, in the latter diminished or suppressed. Seeing, then, that secretions are arrested when the surfaces are full of blood, and abundant often when the surfaces are anæmic, he proceeds to reconcile and explain these apparent paradoxes by propounding the hypothesis that the mucous and cutaneous glands act under the control of a special set of nerves distinct from the sympathetic, and derived from the cerebro-spinal system. He conceives that while the sympathetic regulates the amount of blood entering the gland, the cerebro-spinal nerves, by their action on the gland-cells, determine the intensity of the force by which it is attracted to them; and in this way he shows that glandular activity and glandular inaction are due, in the former case, to a preponderance of cerebro-spinal, and in the latter to a preponderance of sympathetic, nerve force. The cerebro-spinal he calls the positive motor, and the sympathetic the negative motor, nerves of a gland; and it is by assuming an excessive stimulation of the former, causing the gland-cells to draw copious currents of blood through the obstruction of spasmodically constricted arteries, that he ingeniously explains the seemingly paradoxical but common phenomenon of an anæmic skin pouring forth a greasy sweat.

"Dr. Chapman next passes in review the anatomical nerve relations of the various glands, and concludes that the disposition of the nervous system is such as to enable every one of them to receive a positive motor nerve from some part of the cerebro-spinal system, and holds 'that in every case in which glands are not yet proved by anatomical evidence to possess positive motor nerves from the cerebro-spinal system, physiological, pathological, and even therapeutical facts prove that they must be so innervated, while anatomy offers no evidence whatever to the contrary.'

"Having laid down these physiological foundations, Dr. Chapman proceeds to build thereon his remarkably ingenious theory of cholera. To this he might happily have prefixed for motto the axiom of Newton—'Causas rerum naturalium non plures admitti debere, quam quæ et veræ sint et earum phænomenis explicandis sufficiant;' or, still more happily, the assertion of Bernard, who, when speaking of the nervous system, says with a sublime audacity that 'while it is the origin of all the normal phenomena of life, it is also the origin of all pathological action.' This theory will be most clearly expressed in the form of the following propositions:—

"1. All the phenomena of cholera are due to simultaneous hyperæmia of the spinal cord and of the sympathetic nervous system.

"2. All the phenomena of cholera are naturally divisible into two classes, according as they have their origin in the sympathetic ganglia or in the spinal cord.

"3. All active or positive phenomena are due to hyperæmia of the spinal cord.

"4. All passive or negative phenomena are due to hyperæmia of the sympathetic.

"As illustrations of what Dr. Chapman means by positive phenomena, we may mention early diuresis, excessive activity of the intestinal follicles, sweating, cramps, and internal elevation of temperature. As illustrations of negative phenomena may be specified arrest of secretion, aphonia, depression of external temperature, cold breath, loss of cutaneous sensibility, and serous exudation.

"The author neither directly affirms nor denies any primary affection of the blood in cholera. By implication, however, it is denied; and the only logical conclusion to be drawn from his facts, arguments, and illustrations is, that the

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exciting causes of this disease, whatever they may be, exert their primary action immediately upon the nervous system.

"It is only by a close examination of the detailed application of the hypothesis as a means of rendering intelligible the proximate cause of every special symptom that a comprehensive conception of the hypothesis becomes possible. We must content ourselves with the following examples:—

"A copious secretion of pale urine is mentioned by Parkes and others as one of the early symptoms of cholera. Dr. Chapman attempts to prove that this is due to hyperæmia or exalted functional activity of the spinal cord, and maintains that in the onset of cholera the cord is in this condition, stimulating the kidneys to excessive action. But the activity of the organic processes being less energetic than normal, owing to hyperæmia of the sympathetic, which has contracted the arteries, the metamorphosis of tissue is correspondingly lessened, and consequently the amount of the products of organic disintegration usually excreted by the kidneys is less than in health. It is thus, according to our author, inevitable that the conjoint hyperæmia of the sympathetic ganglia and spinal cord results in the discharge of urine paler and more abundant than normal. But as the disease progresses, the negative motor nerve of the kidneys—that is, the branches of the sympathetic distributed to the renal arteries—acts so energetically upon the blood-vessels as almost wholly to cut off the supply of blood to the kidneys. The inevitable result of this is cessation of their functions, or, in other words, the suppression of urine. When patients begin to recover, hyperæmia of the sympathetic ganglia subsides, and in consequence the spasmodic contraction of the renal arteries is relaxed, blood gains free access to the gland-cells, and the secretion of urine returns. If the sympathetic ganglia become very anæmic or much exhausted, or if the positive motor nerve of the kidney be much stimulated, the kidneys become excessively congested, and albumen passes into the urine. In this way Dr. Chapman ingeniously accounts for the increase, decrease, and final suppression of urine in cholera; for the return of this secretion with reaction; and for the condition of the kidneys observable in cases of death during collapse.

"Dr. Chapman's mode of accounting for the rice-water evacuations of choleraics is as follows:—He affirms that, owing to excessive hyperæmia of the spinal cord, the mucous glands of the stomach and bowels are intensely stimulated; that the energy of the positive motor nerves by which they are so stimulated predominates greatly over that of the negative motor nerves of these glands; that consequently copious currents of blood are drawn to them, notwithstanding the constringing force exerted upon their arteries by the sympathetic; that mucus is secreted in enormous quantities; and that the cells of the secreting structures are generated, developed, and shed abortively in extreme abundance. He cites evidence to prove that the flocculent portions of the discharge consist of mucous flakes entangling a countless number of imperfectly formed and prematurely shed cells. With respect to the watery part of the evacuations, he says it is not secreted but exuded from the congested venous radicles of the whole alimentary canal. He adds that, inasmuch as its glands are exceedingly active, they, by the force exerted on the blood in their capillaries, press it forward continually into the intestinal veins, which speedily become extremely distended, and that as the blood is hindered from passing freely through the liver, now in a condition like that of the kidneys, the venous radicles become so distended that their delicate coats can no longer resist the pressure of their contents, the watery part of which escapes into the alimentary canal.

"We are unable to give any further illustrations of Dr. Chapman's method of accounting for the production of the symptoms of cholera. Each receives a consistent and intelligible explanation; but we would direct the reader's attention more particularly to his exposition of the immediate cause of the increase or persistence of heat in the body after death; of the rise of temperature in certain parts before dissolution; of post-mortem muscular contractions; and of the differences in the relative activity of the positive and negative motor nerves in different cases.

"We must pass over the chapter on the causes of cholera, which displays great originality and ingenuity in reconciling and explaining the various modes of action of causative agencies, and a singularly happy power of using his knowledge for the setting forth of new analogies, and the bringing together apparently the most contradictory phenomena for the support of a general law.

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"The chapter on treatment consists of two parts. In the first Dr. Chapman discusses the remedial value of cold and heat; in the second, the value of various drugs.

"If Dr. Chapman's theory of cholera should turn out to be correct, it would seem obvious that the only rational treatment consists in the adoption of such means as will most rapidly and completely subdue the hyperæmia of the spinal cord and sympathetic ganglia and re-establish the healthy equilibrium of the circulation. Dr. Chapman thinks that the time may come when this will be effected by the proper application of galvanism; but he maintains that in the present state of knowledge there is 'no available power of subduing hyperæmia of the automatic nervous centres comparable to that of ice applied along the spine.' In proof of this, he appeals to the testimony recorded by himself and others that 'the leading symptoms of cholera—vomiting, purging, coldness, and cramps—when met with separately as manifestations of other diseases, are capable of being subdued in the majority of cases by the application of ice along the whole or a part of the spine.'

"Dr. Chapman's treatment of cholera consists essentially in the application of ice to the spine and of heat to the general surface: and he is emphatic in insisting that the one should not be used without the other. As importance is attached to the exact manner in which this method of treatment is tried, it is but fair that those intending to test the value of Dr. Chapman's therapeutical proposal should make themselves familiar with what he says before beginning their experiments.

"The work concludes with a record of cases and an analysis of results.

"Whatever may be the final judgment pronounced upon Dr. Chapman's theory of cholera, it must be admitted that he has said nearly all that could be said in its favour. Its strength lies in its comprehensive and simple explanation of seemingly contradictory phenomena by the application of a recognized general truth; its main, and in our eyes great, weakness lies in the denial of any primitive affection of the blood. But even if wholly false, the theory will play an important part in the discovery of that which is true. The work is well-written, methodically arranged, connected in all its parts by a pervading unity of design, and will take a permanent place in the history of the disease of which it treats."

OPINIONS OF THE PRESS

ON

DR. CHAPMAN'S WORK ON

DIARRHŒA AND CHOLERA.

From the "Medical Press and Circular," Nov. 6, 1867.

"The part of Dr. Chapman's work devoted to cholera comprises six chapters, entitled respectively—(1) Definition, History, and Symptoms; (2) Post-mortem Phenomena in Cases of Death during Choleraic Collapse; (3) Pathology of Cholera; (4) Causes of Cholera; (5) Treatment, comprising (*a*) treatment by cold and heat, and (*b*) treatment by medicines; (6) Cases and Results. Chapter I. we must pass over altogether. Chapter II. contains a concise yet detailed description of all the recognized phenomena observable after death during collapse; and every one of these, as well as the several symptoms of the disease characteristic of its successive stages, receives an elaborate and complete explanation in the course of Chapter III., which is distinguished alike by the originality of the views it expounds, the ingenuity and cogency of the arguments by which they are enforced, the faithfulness to well-established facts which by way of confirmation or explanation are referred to at every step, and by the mastery with which the author co-ordinates

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and uses his abundant materials for the establishment and maintenance of his doctrines. . . . .

"Unable to dwell longer on the author's strikingly original exposition of what he calls the negative phenomena of cholera, we select for quotation a few paragraphs which throw a flood of light on certain facts which, thoroughly established, have hitherto remained as utterly inexplicable as they are astonishing. . . . .

"Whatever doubts may be entertained concerning Dr. Chapman's etiology of cholera, there can be none as to the consummate skill with which the facts adverted to in each section of this chapter are ranged and presented so as to establish the doctrine which the author advocates. The various influences enunciated have been adverted to by many previous writers, but, so far as we are aware, the several links in the chain of causation by which these influences produce the phenomena of cholera have never before been exhibited: it is in this respect that the chapter under consideration displays an amount of insight and originality, as well as logical cogency, which cannot fail to commend it to every philosophical mind. It is one thing to see that there is some casual connection between great atmospheric heat, wide ranges of temperament, prolonged marches, and influences operative during the night, on the one hand, and the origination of cholera on the other; but it is quite another thing to show distinctly, and in detail, the *modus operandi* of these agencies; this achievement, in respect to each agent discussed, constitutes the characteristic excellence of Dr. Chapman's elaborate exposition."

From the "Medical Mirror," March, 1867.

"The light thrown by these original ideas on the phenomena in question during the successive stages of cholera must, we think, impress every reader with the conviction that the hypothesis propounded by Dr. Chapman, whether absolutely true or not, at least affords a complete solution of a pathological problem of the first magnitude, and fulfils all the requisities which a true explanation supplies. . . . . It embodies a great amount of novel truth; it is ingenious, well-reasoned, admirably supported, and not only in harmony with, but in advance of, the results of the highest investigations of the time. The subject is treated with real perspicuity and candour, and with a remarkable desire to appreciate every fact at its true value; and the work, as a whole, lacks nothing that is needed to make it a rare specimen of the application of the severest logic, and the most precise manipulation of language to practical science. Indeed, the book is characterized not only by great ability, by originality of thought, by judicial acumen, and by familiarity with the spirit and tendencies of modern research, but also by a rare power of reconciling apparently contradictory phenomena, and marshalling them together for the support of a common purpose."

From the "Journal of Mental Science," Jan. 1867.

"Dr. Chapman applies his well-known views of the pathology of disease, and of its treatment through the agency of the nervous system, with wonderful ingenuity to explain all the phenomena of cholera. . . . . Apart from all peculiarities of theory on the author's part, the present work will be found to contain a clear and complete account of what is known of cholera, and an acute and instructive criticism of the theories of its nature, which have been propounded by different writers."

From the "Popular Science Review," edited by HENRY LAWSON, M.D., Jan. 1867.

"Whatever amount of truth Dr. Chapman's hypothesis may possess, his view is worked out with a display of logical reasoning, formidable facts, and erudition, such as is seldom met with in medical essays. . . . . Of the hundred and one treatises on cholera which have been published during the past year, Dr. Chapman's is at once the most interesting, the most scientific, and the most scholarly."

From the "Indian Medical Gazette," Jan. 1867.

"The section criticizing Dr. George Johnson's castor-oil treatment and lung-capillary theory is very ingenious."

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CASES OF DIARRHŒA AND CHOLERA

TREATED SUCCESSFULLY THROUGH THE AGENCY OF THE NERVOUS SYSTEM, CHIEFLY BY MEANS OF THE

SPINAL ICE-BAG.

8vo, 64 pp., price 1*s.* 6*d.*

From the "Medical Times and Gazette," Feb. 17, 1872.

". . . The author having proposed his method of treatment, has in the next place to determine its actual value in practice; he accordingly completes his case by bringing forward a large body of evidence to show that his method, when carried out by attendants with the minute care he has a right to enjoin upon them is, if not uniformly, at any rate very largely successful. . . . . *We must say that his statements are backed by very strong evidence, used with much knowledge and skill—so much so, indeed, that they cannot be overlooked, but claim our instant attention. . . . . We cannot but be pleased to think that the evidence is most strong in support of that one position in which we are most immediately interested—namely, that the ice treatment is successful.* This surely is the main point, and in the face of the miserable results obtained under the use of other methods, we cannot but form a favourable estimate of the alternative treatment here proposed. . . . . And although we fortunately have no experience of cholera thus treated, yet in fairness to Dr. Chapman, we ought to say that we have found the ice-bag very useful in some other disorders. For example, we have found it strikingly successful in maniacal conditions, when applied to the cervical region; in sympathetic vomiting likewise, and in some other conditions too numerous now to mention."

From the "Medical Press and Circular," 1872.

"Dr. Chapman has the undoubted merit of originating a system of treatment which we believe is of great practical value. We are satisfied of the truth of his assertion, that the condition of the viscera of the thorax and abdomen can be modified to a considerable extent by the external application of heat and cold to the spine. We have ourselves observed the advantages of Dr. Chapman's method of treatment in sea-sickness. In the stages of bronchitis, where the breathing is dry and tubular, Dr Chapman's hot-water bag often causes marked relief, and the increased secretion of mucus from the bronchial tubes seems to be proved by the greater softness of the breathing which may be ascertain to follow in a few minutes by auscultation. . . . . The pamphlet is in a readable form, and shows both great scientific knowledge and practical sagacity."

From the "Edinburgh Medical Journal," July, 1872.

"A readable and persuasive pamphlet. We should like to see Dr. Chapman's views fully tested."

LONDON: BAILLIÈRE, TINDALL & COX, 20 KING WILLIAM STREET, STRAND

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OPINIONS, CHIEFLY FROM THE MEDICAL PRESS,

CONCERNING

THE GENERAL PRINCIPLES

OF

NEURO-DYNAMIC MEDICINE.

From the "Lancet," Feb. 20, 1864.

"Carrying on his experiments, Dr. Chapman erects into a therapeutical system the application of cold and heat to the spine, in various parts, and for different periods of time, and claims for this treatment a philosophical interpretation and wide practice. His brochure (on the 'Functional Diseases of Women') is original in its views."

From the "Medical Times and Gazette," Jan. 23, 1864.

"Dr. Chapman expresses his belief that many maladies not usually held to depend on nervous agency really originate in that manner. . . . . The subject deserves the serious attention of the profession, not only from the high character of the originator, but also from the amount of evidence which he has already brought to bear upon it."

From the "Athenæum," August 16, 1868.

"It is due, we think, to Dr. Chapman to say that he has been the first to make anything like a systematic attempt at applying our knowledge of nervous physiology to the treatment of diseases. . . . . A little thought will show any one how comprehensive a theory of disease is stated in Dr. Chapman's nine propositions, and how universal a remedy is proposed. It is not alone diseases of the stomach and bowels to which heat and cold along the spine may be applied, but to diseases of the whole series of organs that are linked, as it were, to the brain and spinal cord by the action of the sympathetic nervous system. . . . . We think his theories demand inquiry, and his practice a fair trial."

From the "Medical Times and Gazette," June 14, 1873.

"Dr. Chapman aims at something far more than a clinical generalization. . . . . He would tell us of the immediate agency by which each and all of these disorders (of the nervous system) is caused, and, knowing this, he would give us a key to the direct treatment of them all. He does not shrink from saying that here we have a key to disorderly actions of muscles voluntary and involuntary, to morbid actions of glands, to disorders of local nutrition, &c.; that his method is a method of commanding a vast number of very various maladies. Indeed, although no vital function depends primarily and in its simplest terms upon the nervous system, yet in man this system has gained so great a control over all actions in the economy, that to command this system is practically to command the whole body. Dr. Chapman's system is, therefore, something like a panacea, and he asks to have it regarded in that light, and to have it called the doctrine of 'neuro-dynamic medicine.'" . . . .

". . . . It is evident that if Dr. Chapman establishes any great part of his thesis, he has made one of the most remarkable therapeutical discoveries in the history of the art.'

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From the "Medical Press and Circular," June 5, 1867.

"We feel it only due to a most able physiologist to testify to the necessity of submitting his conclusions to the test of experience. . . . . Reasoning on the facts that had been demonstrated by Claude Bernard and Brown-Séquard, that a division of a portion of the sympathetic increases the flow of blood to the parts to which its branches are distributed, while galvanism of the nerve decreases the amount of blood in the same parts, Dr. Chapman directed his inquiries to the possibility of finding remedial agents that would depress or excite the nervous centres. Such a depressant, he concluded, might be found in *cold*, while the opposite condition of *heat* should, *à priori*, act in a manner analogous to galvanism. He put his conclusions to the test by applying heat and cold to the spine, and so satisfied is he with the result, that he has not ceased to urge upon the profession his belief that, by varying the temperature of the nervous centres, we may, at will, diminish or increase the vital activity of those parts which derive their nerve-supply from the portions on which we experiment."

From the "Medical Press and Circular," November 18, 1868.

"Dr. Chapman gives us, in an admirably clear introduction [to his work on Sea-sickness], a synopsis of his views as to neuro-physiology, setting out from the assumption that the sympathetic is the excito-motor nerve governing the vascular system, and that the functional activity of the glands is excited or maintained by a stimulus from the cerebro-spinal axis. The former of these assumptions is now pretty generally admitted since the clear demonstration given of the fact by Bernard and Séquard. The latter proposition is an extension of the views of Bernard, Ludwig, and Pfluger. Bernard proved that the parotid and submaxillary glands receive their nervous supply, on the one hand, from the brain and spinal cord, and, on the other, from the sympathetic; and demonstrated by experiments on animals that when the former are in action the maximum of blood is supplied to the glands and the maximum of saliva secreted, whilst, when the latter are in action, it modifies the volume of the arteries and regulates the supply of blood. Dr. Chapman states that he has discovered that, by applying heat along the spine, he stimulates the glands of the skin and mucous membrane, and by applying cold he restrains or depresses them, thus increasing or arresting their secretions. According to our author, then, the mucous and cutaneous glands act under the control of a special set of nerves derived from the brain and spinal cord, and distinct from the sympathetic. Glandular action, in short, and glandular inaction, are due, the first to a preponderance of cerebro-spinal influence, the second to a preponderance of sympathetic nerve force. He maintains that all glands possess positive motor nerve fibres from the cerebro-spinal axis, even in the cases where anatomy has not discovered such to exist. From these postulates, Dr. Chapman requires our assent to the importance of endeavouring in all cases of excessive discharges from glands, such as diarrhœa, leucorrhœa, bronchorrhœa, &c., to paralyze this cerebro-spinal influence, and thus to inhibit the supply of blood to the glands which causes the discharge."

From the "Medical Press and Circular," April 30, 1873.

"It is, indeed, quite time that the remarkable generalization long since arrived at by Dr. Chapman, and now supported by the vast array of facts and arguments before us, should receive the thoughtful consideration of all those engaged in the treatment of disease. His theory not only explains many morbid phenomena, but suggests a rational mode of treating the diseases on which they depend. This mode has been applied in hundreds of cases with such success that Dr. Chapman is entitled to ask his brethren either to try it in their own practice, or to state their reasons for not doing so, and we venture to assert that those who carefully read this volume (his work on Neuralgia), will hesitate before they measure swords with the author."

From "The Doctor," April 1, 1873.

"Dr. Chapman has expounded a theory that every practitioner ought carefully to weigh, and has enforced his doctrines with a mass of evidence that entitles him to a foremost place among those accurate clinical observers who are helping to advance scientific therapeutics."

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From Professor F. W. BENEKE, one of the Editors of the "Archiv des Vereins für wissenschaftliche Heilkunde."

"It seems to me that Dr. Chapman has done an important service in calling attention afresh to the intimate connection between many morbid phenomena, and morbid conditions of the centres of the nervous system, and to the immediate dependence of the former on the latter—especially on morbid conditions of the ganglia of the sympathetic. . . . . Should the efficacy of his therapeutical measures be verified, even to a small extent only, the knowledge of his general view will conduce to fruitful meditation, and even an only partial confirmation of his therapeutical experiences would be already an extraordinary gain to our power of healing."

In a letter addressed to Dr. Chapman, Professor Beneke writes:—"I think that much can and will be done by the principle of applying cold and heat to the spine, which you have introduced; as far as the heat is concerned, I am fully convinced of its extreme usefulness in certain cases;" and again he says:—"I take always the greatest interest in the very important mode of treatment of certain diseases, which we are owing to you."

From a Paper read to the Surgical Society of Ireland, March 13, 1868, by Dr. J. H. BENSON, Physician to the City of Dublin Hospital.

"This grand object—a new therapeutic agent in our hands by which to control disease—seems to have been accomplished in a very simple way by Dr. Chapman, and by the use of heat or cold to the spine a great number of diseases seem capable of being successfully treated. This power, moreover, extends not only to nervous diseases, universally so called, but also to many of those which, previous to his discovery, were never considered to have had any casual relation to a deranged state of the nervous centres. Such, for example, are bronchitis, diarrhœa, constipation, leucorrhœa, and the usual disorders of menstruation, some skin diseases, and many others.

"Whether this therapeutic agent, then, is a powerful one or not is to be determined by experiment; and I think that those who give it a fair trial will agree that it is, whatever be their opinion of the soundness of the theories which are involved respecting it. In several cases of disease I have used this agent, but generally in conjunction with others, and, therefore, though satisfied in my own mind of its influence for good in those cases, I did not report them, for in order that the remedial value of any given general mode of treatment should be fairly tested, it is obviously desirable that the treatment should be used alone. In two cases, however (one of paralysis and one of suppression of the menses), cold to the spine was used successfully, unassisted by any other agent whatever."

From C. H. ROUTH, Esq., M.D., Consulting Physician for Diseases of Women to the North London Consumption Hospital, and Physician to the Samaritan Hospital.

"Dr. Routh said [at a meeting of the Medical Society of London] he had been to some extent a pupil of Dr. Chapman, and had witnessed his treatment of certain cases at the Samaritan Hospital. The result was the production of certainty in his mind of the truth of Dr. Chapman's doctrine—viz., that the circulation and nutrition of remote parts, or of the periphery of the body, may be increased by the application of heat to the same region; also, that the functional activity of the spinal cord may be depressed or exalted in the same way. These facts, he repeated emphatically, are thoroughly established and indisputable. Being so, they opened up the prospect of a great therapeutical revolution."

From a Letter to Dr. CHAPMAN by Dr. BRERETON of Sydney, New South Wales.

"I have fully satisfied myself of the great value of your discovery—a discovery not of a few isolated facts, but of a *principle* of treatment, capable of most varied application, and, like all principles, likely to lead to further, and as yet unimagined, results."

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From a Letter to Dr. CHAPMAN by Dr. HAYLE, of Rochdale.

"I take this opportunity of thanking you for a very effective additional means of combating disease. Your discovery opens up a wide field of speculation as to the primitive and secondary action of medicines."

From a Letter to Dr. CHAPMAN by Dr. JOSEPH M. O'FERRALL, late Senior Physician of St. Vincent's Hospital, Dublin.

After stating that he had found the spinal ice-bag "very efficient in many cases of painful spasmodic affections seeming to depend on irritation of some portion of the spinal cord," he observes: "The details of these cases are certainly calculated to support your views of the therapeutic effect of the agent in question."

Each of the three immediately following extracts is from a letter written when Dr. Chapman was a Candidate for a certain Professional Appointment:—

From W. B. CARPENTER, Esq., M.D., F.R.S., late Registrar of the University of London; Author of "Principles of Physiology, General and Comparative," and "Principles of Human Physiology," &c. &c.

"I regard Dr. Chapman's medical researches as of the highest physiological interest and therapeutic value."

From R. H. GOOLDEN, Esq., M.D., Oxon., F.R.C.P., Physician to St. Thomas's Hospital.

"I take this opportunity of acknowledging the advantage I have derived, in hospital as well as in private practice, from facts in pathology and therapeutics which the profession owes to Dr. Chapman's labour and acumen, and the correctness of which he did me the honour to demonstrate practically in my wards at the hospital."

From ERNEST HART, Esq., M.R.C.S., Ophthalmic Surgeon and Lecturer, St. Mary's Hospital, and Editor of the "British Medical Journal."

"Dr. Chapman's highly interesting researches on the application of cold and heat to the spine as a curative method, and on the physiological effects of those agents, are very valuable, and probably admit of greater development than they have yet attained, when the limits of the therapeutic application are clearly defined. I have myself, carrying out Dr. Chapman's plan of treatment, met with success in a very intractable form of disease, and the profession is indebted to him for a valuable contribution to the healing art."

From W. H. SANDHAM, Esq., M.R.C.S. Eng. (published in the "Medical Press and Circular"), December 16, 1868.

In his report of a "Case of Melancholia treated by Ice to the Spine," Mr. Sandham says:—"After having read the report of three cases of delirium tremens of a severe character cured by ice to the spine, and having myself had satisfactory results from ice applications in a case of epilepsy, I determined on trying ice in the present case, as directed by Chapman, to whom be all the honour he so well deserves. My patient was in a low state, unconscious, his eyes fixed and vacant, frightening myself and his family, as I feared a fatal termination before morning." Mr. S. then gives the details of his treatment with the spinal ice-bag, and having stated that the patient "is very nearly mentally and bodily as well as ever," adds: "So far the treatment by ice with a view to procure sleep was a complete success, proving the ice application recommended by Chapman to be an invaluable therapeutic agent. . . . . The value of cold to the spine cannot be over-estimated."

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Abstract published in the "British Medical Journal," December 12, 1868, of a Paper entitled "The Therapeutic Application of Heat and Cold."

Read at the Medical Society of the College of Physicians of Ireland, November 25, 1868, *by* Dr. D. B. HEWITT, L.K.Q.C.P., *Physician to the City of Dublin Hospital.*

"After a short preliminary historical notice of the effects attributed to heat and cold as therapeutic agents, Dr. Hewitt proceeded to say that his chief object in bringing the matter under the notice of the Society, was to elicit the experience of any member who might have tried the method recommended by Dr. John Chapman of London. He did not think that a sufficient number of cases had as yet been adduced to furnish matter for an induction so extensive as to be probably true; but the facts as observed by him were quite in accordance with those detailed by Dr. Chapman. He had not seen any thermometrical observations respecting the effect of heat and cold to the spine, with the exception of those which he had brought under the notice of the Society last year. He then detailed several cases in which the use of those agents had been remarkably successful; and a few cases in which they fail to do good. Among the former, he enumerated a case of amenorrhœa, with very violent cephalalgia and spinal pain, in which, after the failure of drugs and dietetic treatment, the ice-bag had been used; the result being that the headache and spinal pain were quite removed, and the menses returned, after the use of a blister to the sacrum, so profusely as to be almost menorrhagic. Ice had been used with excellent effect in two other cases of cephalalgia, but in these cases applied to the head. He referred to three cases of delirium tremens treated by Mr. Hamilton of Steevens's Hospital, by the ice-bag, as recommended by him last year; and he gave some interesting thermometrical observations of a case of delirium tremens lately treated by himself on the same plan. When the ice was placed along the spine, the temperature of the axilla rose, and the pulse was increased in frequency, while the profuse sweating was checked; afterwards it was thought well to put the ice to the head, when the result was diminished frequency of pulse and a fall in the temperature. A case of obstinate vomiting, occurring during pregnancy, which had resisted all treatment, was quite checked, and even nausea was removed by the application of ice to the lower dorsal and upper lumbar spines. In a case of spinal myelitis, the temperature was raised, the pulse diminished in frequency, while it became fuller and stronger, and hyperæsthesia of the upper extremities was removed by the use of ice, and the paralysis was greatly diminished. In a case of chronic hemiplegia, it had failed to relieve the rigidity in the flexor muscles of the fore-arm, or to benefit the paralysis; and in a case of chorea the result was not encouraging. He referred to a case of chorea, published by Dr. J. H. Benson, in which this method was successful, after the use of a brisk purgative. In conclusion, Dr. Hewitt gave his experience of the use of the hot-water bag in some cases of bronchitis. One of these was a case of chronic catarrh; and, though no expectorant was given, in three or four days the expectoration had become much more abundant, the dyspnœa was greatly relieved and the cough considerably lessened. The thermometer revealed, on one occasion, after two hours' application of the hot water, a decrease of seven-tenths of a degree; and on another day the temperature decreased at first, but subsequently rose.

"Dr. Benson, jun., referred to several cases recently treated by him with the ice-bag. He believed the manner of its application, and the length of time necessary for its use, had not been sufficiently understood, and hence its frequent failures as a remedial agent. Dr. Atthill strongly advocated its use, and related a case of menorrhagia, in a girl aged fifteen, in which he had applied it with remarkable success. Dr. Belcher approved of Dr. Hewitt's paper, and noticed the treatise of Esmarch, 'On the Use of Cold in Surgical Practice.' Mr. Croly gave an interesting account of a case successfully treated by him with the ice-bag. Mr. Thorpe Porter had cured a case of hæmorrhage with it."

From a "Traité Théorique et Pratique d'Hydrothérapie," par Dr. BENI-BARDE, Paris, 1874.

"Whatever theory we may adopt, the therapeutical effects of the spinal ice-bag in the treatment of cerebral anæmia are evident, and we can affirm that, in a great number of cases, they are very salutary" (p. 793).

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Referring to the treatment of vomiting, this author writes:—"Of all the means employed, Dr. Chapman's spinal ice-bag has appeared to us the most convenient, and certainly the most efficacious. . . . . This method is inoffensive, easily practised, and, in the majority of cases, extremely useful. We know cases in which its therapeutical action is really marvellous. . . . . We can affirm that the ice-bag has rendered us real service in several difficult and alarming cases" (p. 897).

Adverting to amenorrhœa, Dr. Beni-Barde remarks:—"The ice-bag so acts on the blood-vessels by means of the vaso-motor nerves as to force them to dilate, and consequently to render the circulation completely free. This paralytic effect extends to the vascular nerves of the feet, and manifests itself by the generation of heat in the extremities. We know amenorrhœic women having an intolerable coldness of the feet even in summer, who, under the influence of the lumbar ice-bag, have experienced at one and the same time the re-establishment of the menstrual function and the function of calorification" (p. 995).

Respecting menorrhagia, he says:—"We can affirm that Dr. Chapman's spinal warm-water bag renders real service in cases of hæmorrhage" (p. 1003).

From Dr. GUENEAU DE MUSSY, Membre de l'Academie de Médecine, Médecin de l'Hotel Dieu.

This distinguished physician published in *Les Annales de Gynécologie* for July, 1875, some remarkable cases of metrorrhagia arrested by the application of heat to the lumbar region. One of these cases was that of a woman who suffered very severely during six months from that disorder. She was for some months in the Hospital St. Antoine under the care of Dr. Brouardel, who "made use of the most varied remedies, comprising warm baths, astringent injections, cauterizations of the womb, without effecting the arrest of the hæmorrhage. She then became discouraged, and obtained admission to the Hôtel Dieu. . . . . She continued to lose blood copiously, and several times a day she expelled large clots. Examination of the womb revealed nothing capable of explaining the cause of the metrorrhagia. . . . . I had recourse to the series of remedies which had already been adopted by M. Brouardel, after having tried in vain sulphate of quinine, and blisters to the hypogastrium. . . . . I introduced solid nitrate of silver into the cavity of the neck of the womb, leaving it to dissolve there." All the means used having proved inefficacious, "I then decided to employ the remedy advocated by Dr. Chapman, and by means of his india-rubber bag," heat was applied to the lumbar region. "The applications were renewed every third hour. The next day, February 16, the hæmorrhage had considerably lessened; the 17th, it was completely stopped." At the end of a month the menses recurred, and "soon assumed the appearance of a veritable hæmorrhage. . . . . The patient swooned several times. . . . . I applied the hot-water bag, and in thirty-six hours, as on the previous occasion, the hæmorrhage was completely stopped."

Dr. Gueneau de Mussy relates another case of a woman who was admitted under his care into the Hôtel Dieu on account of a severe metrorrhagia, probably due to abortion: "On the 25th of February warm water was first applied to the lumbar region by means of Dr. Chapman's water-bag. On the 26th the flow had diminished in a sensible degree, and on the 27th it had completely ceased."

Dr. de Mussy also mentions a case of suffocating catarrh complicating pulmonary tuberculosis, in which there were sub-crepitant rales throughout the whole chest with extreme dyspnœa, and he states that the patient "was quite relieved of this complication in twenty-four hours after the application of Dr. Chapman's hot-water bag."

In concluding his paper Dr. de Mussy observes:—"The testimony of Dr. Atthill of Dublin confirms the assertion of Dr. Chapman, and authorizes the conclusion that the subject in question is not merely one of those illusions that are too often encountered in the case of inventors of a therapeutic method."

From the "Medical Mirror," April, 1867.

"In his pamphlet, 'Functional Diseases of Women,' Dr. Chapman gives thirty-two cases which he had treated successfully either by cold or heat, or by both to-

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gether, applied along the spine. Six of these cases were cases of defective and painful menstruation, which were cured by the application of the spinal ice-bag. . . . . One of the foregoing cases exemplified the curative power of the spinal ice-bag over leucorrhœa as well as deficient menstruation.

"In a very interesting section, entitled 'Coldness of the Feet; its Relation to Functional Diseases of the Womb, and Its Cure by means of Ice,' he gives a number of cases proving that its application along the lower third of the spine causes the lower extremities of patients who had suffered habitually from coldness of the feet to become permanently warm. In our last number we published reports of two cases illustrative of the power of the spinal hot-water bag in arresting menorrhagia. We need, therefore, only allude to the six cases of this malady treated successfully by means of heat, and reported in detail in Dr. Chapman's pamphlet. He also gives a very curious case tending to prove that the development of the mammæ may be influenced by cold and heat applied to the back.

"In the 'Journal of Mental Science' for April, 1865, Dr. Chapman gives a remarkable case of the sickness of pregnancy which he had effectually controlled in the same way. The lady in question had had several miscarriages brought on by the violent retching incident to her sickness, which was always continued until the ovum was expelled; but, owing to careful treatment, by means of the spinal ice-bag, she was enabled when last pregnant to retain her food, and to carry the child to the full term.

"There can be no doubt that any man who will, with an unbiassed and philosophical mind, consider Dr. Chapman's cases, must admit that he has made a discovery which promises to do much good to suffering humanity. . . . . Not only is the result of ice effective towards increasing the vitality of the uterus, and promoting the period, but the pain is diminished. The dysmenorrhœa is, in fact, cured. . . . . It would be well, for the patients who are supposed to be suffering from mechanical dysmenorrhœa, if before the uterus was cut with hysterotomes, a proceeding in the hands of some practitioners not always free from danger, that Dr. Chapman's plan was made the preliminary treatment. . . . . Section III. is the first philosophical essay that we seem to have read on cold feet. . . . . We must with him admit that ice applied to certain parts of the spinal cord will promote the menstrual flow, and produce warm feet; and the nine cases mentioned are fully confirmatory of this view. . . . . It cannot be doubted but that the obstetric physician may derive from these views of Dr. Chapman's great assistance in the treatment of many uterine cases, hitherto almost incurable, always tardy and tedious, both to patient and himself. Section IV. treats of hæmorrhage and menorrhagic pain cured by means of heat. We are almost better pleased with Dr. Chapman's results here than with the former. The correctness of *former* results following the use of ice, is greatly enhanced when we find exactly the opposite effects following the use of heat applied to the spinal centre. We cannot fail to be struck with the rapidity with which the menorrhagia is controlled. Ergot has not the same power; and the advantages of this plan in many cases of fibroids, change of life, flooding, is very conspicuous. We have read Dr. Chapman's book with pleasure, and we are pleased with his manner of working. It is clear he has made a very useful discovery and more useful applications, and he knows it. He is not an enthusiast simply. He is a devoted learner and a modest teacher."

From Dr. ROUTH'S Speech at the London Medical Society, March 18, 1867, reported in "Medical Press and Circular," April 3, 1867.

"He said the method had been tried in a case of what he called 'convulsive action of the stomach,' associated with pregnancy. The patient suffered from sickness continually, for nearly three months; it was no use giving her drugs, for they came up again directly. She had been supported to some extent by injections per anum, but was in such a state of exhaustion that the question of inducing abortion, in order to stop the sickness, was entertained. By Dr. Chapman's advice, the spinal ice-bag was applied. The immediate effect was the production of refreshing sleep; by continuance of the treatment, the sickness steadily and completely subsided, and the patient is now well and gaining flesh. Dr. Routh said he had tried the method in a case of profuse menorrhagia; after the double-column hot-water bag had been applied during an hour, the flow ceased.

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PRELIMINARY OBJECTION TO THE APPLICATION OF ICE ALONG THE SPINE.

"A great preliminary objection to Dr. Chapman's therapeutical method consists in the reluctance which patients, when they hear of it for the first time, very naturally feel to be treated by the application of ice along the spine. In order to overcome this feeling by showing its groundlessness, Dr. Chapman cites the testimony of a considerable number of patients who have been treated by ice in cases of sickness, diarrhœa, neuralgia, and various other disorders, and certainly there can be no mistaking the language of the patients themselves written down from their own lips. The following expressions are samples of the kind of testimony given by a large number of patients :—'The ice is beautiful; I don't think I shall ever be able to do without it—it is so comforting.' 'I find the ice very agreeable; I look for it, and would like to have it on longer each time.' 'I go to sleep with the ice on; it's astonishing how pleasant it is.' In a report of a case of diarrhœa treated successfully by ice to the spine, the director of an hydropathic establishment remarks: 'One thing has much struck me—viz., the liking that sensitive chilly patients have for the cold bag to the spine, although frightened to think of it before they make a trial;' and Dr. Druitt, who, it appears, examined several patients treated by Dr. Chapman, says: 'I learned from all the patients that the treatment had made them more comfortable—I mean as regards their general feelings of health and animal sensations, without reference to the relief of particular symptoms.' We may add that in forty out of one hundred cases given in this volume* the records of them prove that instead of inducing a feeling of coldness, the use of the spinal ice-bag so improved the circulation as to cause patients who had habitually suffered from that feeling to become permanently warm."—*The Doctor*, April 1, 1873.

* "Neuralgia, and Kindred Diseases of the Nervous System."

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From the "Medical Times and Gazette," June 14, 1873.

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From the "Medical Press and Circular," April 30, 1873.

"The work before us, from the pen of Dr. Chapman, presents an elaborate analysis of the symptoms of the whole group of neuralgic diseases, and an interpretation of those symptoms according to the theories of neuro-pathology to which the author has been led by observation of the effects of the particular system of treatment in connection with which his name is chiefly known to the medical profession. The careful study of the natural history of the disease, and of the complications which it presents, evinces both careful and continued observation, and a thorough acquaintance with the literature of the subject. The facts and opinions of every writer on neuralgia, English and foreign, past and contemporaneous, are laid before the reader fairly but critically, and in a manner which renders the work a useful contribution to medical literature, quite apart from the tenets, the presentation of which is its special object, and which we consider it the duty of the profession to put to the test.

"It is, indeed, quiet time that the remarkable generalization long since arrived at by Dr. Chapman, and now supported by the vast array of facts and arguments contained in the volume before us, should receive the thoughtful consideration of all those engaged in the treatment of disease. His theory not only explains many morbid phenomena, but suggests a rational mode of treating the diseases on which they depend. This mode has been applied in hundreds of cases with such success that Dr. Chapman is entitled to ask his brethren either to try it in their own practice or to state the reasons for not doing so, and we venture to assert that those who carefully read this volume will hesitate before they measure swords with the author. The work is an elaborate and well-sustained argument for the truth of the author's theory, and constitutes so important a contribution to rational therapeutics, that we proceed to furnish our readers with a detailed account of its contents."

From "The Doctor," April 1, 1873.

"This work, though an exhaustive treatise on neuralgia and kindred diseases of the nervous system, puts forth claims which, if conceded, would involve nothing less than a revolution in both pathology and therapeutics. A critical sketch of 'Pathological Theories of Neuralgia' is given in chapter II.; in chapter VIII. Dr. C. B. Radcliffe's 'Theory of the Genesis of Pain' is subjected to a searching critical analysis; and in chapter XI. Dr. Anstie's 'Pathology and Etiology of Neuralgia' is also elaborately summarized, while every argument in support of it is submitted to merciless scrutiny. The significance and bearing of every pathological fact adduced by these physicians in support of their respective, and, indeed, kindred theories, are carefully examined, and after weighing the whole of the evidence presented in favour of these theories, our author sums up its value in each case in a concluding paragraph, and gives judgment against them. As examples of critical analysis, logical acumen, complete mastery of the subjects dealt with, and crushing confutation of the doctrines discussed, the chapters deserve special attention.

"The wide range of subjects strictly cognate, however, and having a direct bearing on the main theme, which are discussed in this chapter [On the Pathology of Neuralgia], and the thoroughly exhaustive and scientific way in which they are handled, present striking evidence of the author's extensive knowledge, profound as well as logically consistent thought, complete familiarity with every aspect of the problem dealt with, and of his power of skilfully arranging all the facts and arguments relating to it, so as to converge them to one issue, and thus to lead his readers irresistibly to the conclusion, which it is the aim of this chapter to establish. A glance at the topics discussed under the head of one group, namely, Group vi. for example, will suffice to give an idea of the extent of area traversed, though, of course, it can convey no notion of the thoroughness and originality with which the work is done.

"We have only to add that the work, as a whole, reflects credit on the medical literature of the day. In a lucid style, and with logical precision, Dr. Chapman has expounded a theory that every practitioner ought carefully to weigh, and has enforced his doctrines with a mass of evidence that entitles him to a foremost place among those accurate clinical observers who are helping to advance scientific therapeutics."

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